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#THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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TO OUR READERS.

We are happy to announce that a Biography of the celebrated James Watt will appear in our next Number.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Historical Eloge of Antoine-Laurent Jussieu.[*] By M. Flourens, Perpetual Secretary of the Academy of Sciences of France.

The family of Jussieu proceeded originally from the small town of Montrotier, situated in the midst of the mountains of the Lyonnais. One of the members of this family went to Lyons about the year 1680, and established himself as an apothecary. He married there, and was the father of sixteen children, three of whom, Antoine, Bernard, and Joseph de Jussieu, became three of the most distinguished botanists of the eighteenth century. The oldest of this numerous and gifted family was called Christophe, and he was the father of M. Laurent de Jussieu, who had the honour of conferring additional celebrity upon the name his uncles had left him, and the felicity, not less rare, of handing it down to a successor who maintains its renown. In this family a taste for botany seems, for nearly two centuries, to have been hereditary, in the same way as a mathematical genius has, for nearly the same period, characterized the family of Bernouilli.

Antoine de Jussieu, who commenced the celebrated career of the family, was a botanist almost from his infancy; at the age of fourteen, making excursions in the neighbourhood of Lyons, and throughout the surrounding provinces. At the age of eighteen he went to Montpellier, where he studied under Magnol, who introduced the term *families*,—a very happy expression, though at first little understood, for the affinities, and, we may say, the parentage of plants. At the age of twenty-four he succeeded to Tournefort, the greatest botanist of his own age, or, perhaps, of any other time; for he was the first to establish the fundamental principles of the science, as Linnæus, at a later period, conferred upon it its nomenclature. Antoine being required to devote a large share of his attention to the laborious duties of the healing art, in which he excelled, did not perform for botany those distinguished services which his ready and precocious genius gave reason to expect. He, however, called his second brother Bernard to supply his lack of service, and by this judicious step probably did more for the science, than if he had devoted the whole of his own energies to its prosecution. After having thus assisted Bernard to commence his brilliant career, he did the same kind office, and in the same way, for his younger brother Joseph, whose life was as agitated as that of his elder brother's was tranquil, and who abandoned his native land for Peru in the year 1735. He then accompanied, in the character of botanist, the scientific expedition which the Academy sent to the Equator, there to measure a degree of the meridian, and thus, by direct experiment, to settle the famous and long-debated question of the figure of the earth. Joseph Jussieu was a bright example of all that courage, and patience, and devotedness to science could accomplish; nor less so of the sad characteristic of the scientific heroism of modern times, which, in every quarter of the globe, exhibits the tombs of its most assiduous votaries. Detained at first, by the curiosity which those rich and novel regions inspired; and afterwards by the inhabitants, who, afflicted with a fatal epidemic, would not dispense with the services of so able a physician; he returned to France, after thirty-six years of most fatiguing service, broken down alike in body and mind, having even become oblivious of what he had accomplished,—thus establishing by so many trials and misfortunes, his claim to the title which Condorcet conferred on him, *the Botanical Martyr*. Of these three brothers, the only one who produced on botany, and, through botany, upon the whole of natural history, one of those striking influences which mark an epoch in science, was Bernard. Whilst all the other French botanists, commencing with his brother Antoine, were, with timid step, following the traces of Tournefort, he opened up a new path previously untrod, and in which no one has advanced so far as his nephew, M. Laurent de Jussieu, the subject of the present memoir.

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[*] Read before the Academy, August 1838.

Antoine-Laurent de Jussieu, the nephew and successor of Bernard, was born at Lyons on the 12th of April 1748. As soon as his elementary studies in that city were terminated, his uncle invited him to Paris, where he arrived in July 1765, in his eighteenth year. He was thus at once put under the protection of the man who, since the demise of Tournefort, swayed the sceptre of botany in France, and whose only rival in Europe was Linnæus; he was a wonderful man, whose name is a household word throughout the scientific world, and who, notwithstanding, has scarcely written any thing. But if this eminent individual wrote little, he thought much; and spent his life in meditating upon one of those questions which comprehend almost every other in a science; he resolved the problem of *arrangement* in natural history, and solved this fundamental problem in the middle of an age, whose efforts of every description have prodigiously advanced the domains of human intelligence.

At the time that the young Jussieu came to reside with Bernard in Paris, his uncle Antoine was dead, his uncle Joseph was still in Peru, and his illustrious aged relative lived almost alone. Residing in a small house in the *rue des Bernardins*, he went out only to mass, to the academy, and to the *Jardin des Plantes;* he was almost always engaged in deep meditations, and allowed them to be interrupted only—if interruption it might be called—by intercourse with a few friends, selected from the choice men of the day, such as Poivre, Lemonnier, Duhamel, Malesherbes, &c. Such was the retired life of Bernard; and to this simplicity of manners, and a taste for free and continued meditation, in which, by a peculiar trait of his character, he seemed rather to allow ideas to suggest themselves than to search for them, he conjoined a most remarkable regularity in his habits. Every thing in his mansion was subjected to the most exact regularity and order, to what might be designated the very *essence of arrangement.* Every thing was done every day, at the same moment, and in the same way. Every meal was at a fixed and invariable hour; the supper hour was nine, and when Laurent went to the theatre, he never forgot to calculate the precise number of minutes which it would take to hurry back to the parlour, so that he might enter the one door as his uncle opened the other. We may here mention another anecdote which exhibits Bernard's character in another point of view. He was in the habit of depositing in a certain chest that portion of his revenues which was not required for his current expenses; one day he required to make an extraordinary outlay; he opened his coffer, where he found 40,000 francs. On settling this demand, the box was again shut, to be opened only after his death, when nearly the same sum was again discovered. We may here add, that he used his intellectual wealth very nearly in the same way as his secular; he allowed it to accumulate with the same regularity, and the same steady constancy, and yet with a kind of indifference, till, on one occasion, he opened it out, and traced the sketch of his *Ordres Naturels,* the immortal monument of his genius. After this he again left it to accumulate afresh, and at his death he transmitted it a legacy to his nephew, the most precious portion of his inheritance.

Bernard spent nearly the whole of his time in reflection, and usually in a sitting posture. The uncle and nephew worked throughout the whole day in the same apartment, and with little or no conversation. In the evening the nephew read to his uncle, who, in his turn, communicated all his own views and reflections. It will at once be perceived that the impressions received from a man of this stamp, must have exerted a powerful influence upon the character, not less than the genius, of young Jussieu; hence there was the same simplicity of habits, the same perseverance in labour, the same constancy in the development of one and the same great leading idea; never did two men appear more to be merely the continuation the one of the other—the two successive ages, or phases of one life.

After five years thus spent with his uncle in such laborious study and intimate intercourse, Laurent took his degree in physic, and in his twenty-third year became the assistant of Lemonnier in the Chair of Botany, in the *Jardin des Plantes.* As soon as he commenced to discharge the duties of professor, the influence of Bernard upon his reflections must have acquired new power. He consulted him in his various difficulties, and submitted to him his doubts; though, at the same time, we ought to add, that, even in those discussions which he then originated, he was often less influenced by scientific curiosity than by filial affection. For, since the death of his brother Antoine, Bernard had become sadly melancholy, and shortly after this he lost his sight. The only links, then, which bound the venerable old man to life, were those supplied by a young man, who unceasingly watched to reanimate, by searching and difficult inquiries, a genius which was wont to be ardently bent upon meditation.

In the year 1773, a vacancy occurred in the Academy, and Bernard urged his nephew to become a candidate for the honour; but Laurent had hitherto published nothing. It was, therefore, necessary for him to prepare some memoir, and for the subject of his first work he chose "*The examination of the family of the Ranunculi.*" The subject selected was of no great moment, for, whatever it might have been, it supplied an occasion for manifesting his powers, and developing his enlarged ideas. Hence, it was, in fact, by a powerful reaction upon his uncle's opinions that he conceived those ideas under a new form, a form which was his own; and that he, in his turn, conferred upon them the impress of his own thoughts and of his own genius. He used often to mention that it was this memoir which made him a botanist; that the curtain was then raised,—le voile s'était levé—was his expression; and that then, for the first time, he recognised those grand principles, the demonstration of which henceforth became the constant aim of all his efforts and researches.

This memoir produced the strongest possible impression. It introduced to notice a completely new set of ideas. After its publication, a new element,—the constituent principle of the Natural Arrangement—then received its place in science, and speedily effected a change upon its whole character. Up to this period, and particularly since the time of Linnæus, science had very much turned upon nomenclature; but now, and in obedience to an impulse which conducted it nearer to its true object, the nature of beings, it caused the study of characters to take the place of the study of nomenclature. "Nomenclature," says the author himself, "ought not to be neglected, but the discovery of characters is the more important part of botany." He established the principle, that all characters are not of equal value; that there are some which are general, and some particular, some constant, and others variable, some primitive, and others secondary; sometimes a single one is of more real value than many; and hence the axiom, that we are not to *count* characters, but to *value* them.

Characters are the *indicatory signs* of the relations of beings. In every organised existence, whether vegetable or animal, each part has a necessary relation to all the other parts. Consequently we may judge of them all, from any of them; and the parts which are taken for the *signs* of the others,—the parts by which we judge of the others, are those which are denominated *characters*.

Naturalists began by seeking for these *characters*—these *signs*—in all parts nearly indifferently; ere long, however, they recognised that these parts have nothing like the same virtue in the processes of uniting, or of distinguishing, different beings. Hence, then, arises the process of computing or *calculating characters*, and this *calculating* gives the solution to the problem of arrangement. About the middle of the sixteenth century, Gessner proposed to derive the principal characters of plants from the organs of fructification: this was the first step,—the pre-eminent importance of the seed, demonstrated by Césalpin, was the second.

Perhaps the most interesting problem in the whole science of vegetable physiology has been the determination of the peculiar function of each part of the flower. As every one knows, a flower is composed of many parts. In the middle is the *pistil* or female organ; around the pistil are the *stamens*, or the male organs; next the *corolla*, the more brilliant and coloured part of the flower, what is called the flower itself, by Tournefort, surrounds the stamens; and the *calyx*, the prolongation of the external covering of the bark, in other words, the *epidermis*, envelopes all these parts. A century and a half after the time of Gessner, Tournefort was still ignorant of the true use of the stamens; he actually denied this use, which was correctly assigned to them by Vaillant. The ideas of Vaillant upon the sexes of plants, which have since become popularly known by the ingenious system of Linnæus, were confirmed by the accurate experiments of Linnæus himself, by those of Gleditsch, and of Koelreuter, and thus the *physiological* problem was settled.

It is to M. de Jussieu that we owe the solution of the problem which relates to *arrangement.* He noticed that the *corolla* and the *calyx* were wanting in a great number of plants. The *pistils* and the *stamens* again were more essential, were the producing parts of the embryo—of the new being—and were always present; but, taken separately, these organs supplied only incomplete alliances—natural alliances, and complete ones, are furnished only by the two organs taken together, and considered as regards their *respective insertion. The insertion, then, of the stamens in the flower,* forms the first character. The primary character of the seed is drawn from *the lobes of the embryo*—the new existence. These lobes are the first leaves of the new plant, the organ which supplies to it its first aliment, or at least prepares it for it. It will readily be understood, then, to use a happy expression of M. de Jussieu himself, that the remarkable and simple differences which are observed in these first organs must exert a peculiar influence upon the general development of the plant, and upon its whole organization. All the other parts of the seed, those which are strange to the new being and the parts of the seed properly so called, such as the *seminal envelopes*, the *perisperm*, &c., are only secondary portions. This memoir of M. de Jussieu, in which he laid the foundation of the science of characters, bore the date, as we have stated, of the year 1773; and this memoir opened to him the doors of the Academy.

Next year, 1774, he presented to the same learned body another paper, which was more extended and complete, and in which his leading ideas were again reviewed, somewhat differently arranged, and detailed with more clearness and precision. The occasion of this second memoir was the following:—The arrangement of Tournefort, which this great botanist himself established in the *Jardin des Plantes,* still prevailed there in the year 1774, in spite of all the changes which had taken place in the science; and the necessity of some alteration was very generally felt. At the same time, the number of the species of plants, which had been acquired during this long interval, had vastly increased, and the former space was very far from being sufficient. It was under these circumstances that Buffon proposed a plan for enlargement worthy of the epoch on which his own name confers a date. He explained his project to Louis XV. who was fond of botany, and who adopted it. The garden was doubled in its dimensions; and from that time it became necessary that the whole portion which was devoted to the *School* properly so called, should be replanted. All that remained to be determined, was the method which was to be pursued in this new plantation. The method of Turnefort could no longer be maintained, at all events, as a whole, and more especially since the prodigious progress Linnæus had made, first in the determination of genera, and, then, in the simplification of the nomenclature. Nor could the system of Linnæus, in many respects so ingenious, with more satisfaction be adopted, because, in fact, it was more removed from the natural order, even than that of Tournefort. Two plans, therefore, could only be thought of, either to correct the one of these systems by the other, or to establish a new one; and this dilemma formed a barrier to any immediate progress.

The new system proposed by M. de Jussieu was a sage combination of the celebrated labours of Linnæus, of Bernard de Jussieu, and of Tournefort. From Linnæus he borrowed the *genera*, the *species*, and the *nomenclature;* from Bernard the *orders*, or the *natural families;* and finally, from Tournefort the means of multiplying the *classes* of Bernard without disturbing his *orders* or his *families*.

The *genera* of Linnæus were the most precise which had hitherto been established; his *species* were the best determined, and his *nomenclature* was admirable. This nomenclature which, as it regarded every plant, reduced the long phrases of Tournefort and of Gaspard Bauhin to two words, the name of the *species* and the name of the *genus*, constituted alone, a very great reformation of the science. Nevertheless, when its introduction was proposed into the *Jardin des Plantes*, a difficulty presented itself. The prejudice of Buffon against the technical part of the classification was notorious and serious, and at first he entered a protest against the introduction of the Linnæan names; M. de Jussieu's first task, then, was to convince the eloquent zoologist that these names constituted one of the happiest changes which had been effected in Natural History; and he added that the Garden at Paris should not be second in any improvement whatever; and to these convincing arguments Buffon soon yielded. The *Jardin des Plantes* received, at one and the same time, the nomenclature of Linnæus, and the natural orders of Bernard.

These *natural orders*, such as Bernard had recognised them, were comprehended in seven classes. Laurent perceived the necessity of multiplying them, and he precisely doubled their number, making fourteen. The *lobes of the embryo* supplied the three first: hence the famous division of the whole of the vegetable kingdom into the *acotyledonous*, *monocotyledonous*, and *dicotyledonous* plants; and the *insertion of the stamens* upon the pistil, upon the support of the pistil, upon the calyx, or upon the corolla, supplied the rest. Thus, two orders of character, the former drawn from the embryo, and the latter from the relative insertion of the different parts of the flower, supplied all the classes. Characters, of less and less importance, afforded other groups, viz. the families, the genera, and the species: the whole of the groups subordinated themselves in the arrangement, as the characters themselves did in nature; and the fundamental and constituent principle of the arrangement, derived from nature itself, is *the relative importance of the characters.*

But this *relative importance of characters*, the basis of the whole edifice of the arrangement—how was it to be estimated in its turn—how was it to be valued with certainty? Here two methods present themselves, which are equally certain. One, which is founded upon reasoning, directly infers the importance of a character from the importance of the part which supplies it. In a vegetable, every thing contributes to the formation of the flower; every thing in the flower contributes to the formation of the embryo, or the new existence; the formation, then, of this new existence, or being, of the embryo, in short, is the aim and end of all other vegetative functions. *It is, therefore*, says M. de Jussieu, *in the embryo that naturalists ought to look for their primary characters.*

When this method, founded upon reason, and which may therefore be designated the *rational method*, fails, and it very soon fails in botany, the author supplies its place by another which is purely *experimental*, but nevertheless equally sure, and which is, moreover, unfailing. In default of a function which is not known, or which is but little known, or which, at best, is not sufficiently known to declare the importance of the organ, he determines the importance of the organ by its constancy. Nor is this all. As it is with an organ itself, so is it with each circumstance appertaining to an organ; the most constant circumstance, in other words, the most general, is always the most important. Linnæus made the stamens the base of his system; their number, attachment, union, proportion, and situation; he considered all, he made use of all, without perceiving that, among all these characters, one only was of importance, and because it alone was constant, namely the *attachment* of the stamens, or their insertion. Tournefort again founded his system upon the corolla. He regarded its presence, its absence, its situation, division, and form, and employed all these characters, which are variable, whilst he altogether overlooked the character drawn from the attachment of the corolla, the one which alone is constant. The natural order escaped the observation of these two great men, and, as we have noticed, from the same cause, because they were both ignorant of *the relative importance of characters*. It will, moreover, be found, if we direct attention to the progress of botanists since the time of Gessner, that all those who have succeeded in their attempts, having established some fragmentary portions of the natural order, have followed, though unknown to themselves, the principle of *the importance of characters*. Finally, as is generally known, there are some natural families which are as it were ready made to our hands, as the *Gramineæ*, the *Compositæ*, and the *Umbelliferæ*. In the study, then, of these families, every character which varies in one of them is made subordinate and secondary, and the primitive character, in other words, the essential one, or which may properly be called *the important character*, embraces the whole family.

There is, therefore, an order, a gradation, and a subordination in the characters; and the true problem is the classification of these characters in the same method that the existences themselves are, in their turn, classified; and this brings under our notice a phase of the science which is altogether new. Bernard de Jussieu, who introduced the principle of the *relative importance of character* into the classification of plants, never sufficiently disengaged it from the details of practical botany; but Laurent exalted it, and gave it a conspicuous place as a theory, and by this transformation itself he generalized it, and demonstrated all its important bearings; he consummated the great revolution which his uncle commenced, and created the philosophy of arrangement.

When M. de Jussieu wrote those two memoirs on which we have been dwelling, the first germs of all that he has subsequently accomplished, both his uncle Bernard and Linnæus were still alive. Shortly after, however, they paid the debt of nature; Bernard, in the year 1777, and Linnæus the year after. From that time, the first place in the science was vacant, and every one perceived that M. de Jussieu was about to fill it; and of this he himself was perfectly conscious. In keeping with this remark, I find, in one of his letters written at this period, these remarkable words:—"Circumstances are ever occurring by which a man ought to profit; and an occasion of this sort occurs in my experience, which it would be wrong to neglect. We have lost, within the period of three months, the three first botanists in Europe. M. Haller in Switzerland, Linnæus in Sweden, and the third in Paris. There will be a glory in succeeding them, and in obtaining for France that Primacy which strangers have disputed." These words betray the opinion which he entertained of his own powers; and they were immediately exhibited in another and more important manner, namely, by the enterprise which at that time he undertook, which was nothing else than to subject the whole of the vegetable kingdom to those principles which he had propounded in his memoirs;—an immense undertaking, whose result has been that great work upon *Les*

Familles des Plantes, from the publication of which dates the new spirit which, at the present time, animates all those who are engaged with the alliances and the classification of beings. The natural method of arrangement was the grand object to which all the efforts of naturalists had tended, previous to its discovery; and, once discovered, it has since become their polar star in every subsequent effort.

The ancients, if we except Aristotle, and Aristotle alone, did not concern themselves with the relations of beings. They made researches in natural history, and especially in botany, only on the score of utility, and studied vegetables only as they bore upon domestic economy, and upon physic. The order, the relation of species, the arrangement, which is the expression of this order and these relations,—all, in short, in botany, that was purely scientific, escaped them; and it was scarcely possible it could be otherwise, as so few plants had been brought under their cognizance. They amounted only to 500 in the days of Theophrastus, to 600 in those of Dioscorides, and to 800 in Pliny's time.

The natural order,—in other words, the true order of beings,—has its materials dispersed over the whole surface of the globe. It may be compared to an edifice, of which we have only the disjointed and overturned fragments; nay, of which we are very far from having all these, and of which we are, nevertheless, solicitous to determine the reconstruction. It will readily be understood, that the greater the deficiency of the fragments, the greater will be the difficulty of the restoration; and that this deficiency may still be so great, that our object will be unattainable; and that the only way in which we can be rigorously certain that it is perfectly accurate, would necessarily be the possession of the whole.

At the close of the middle ages, astonishing discoveries rapidly succeeded each other; and the most wonderful of all was the discovery of the New World. The curiosity of mankind, awakened by these great events, excited them to bolder and more energetic researches. The sciences were reanimated, extensive voyages were commenced, and the ascertained number of beings augmented with a rapidity which increased its own speed, and in a ratio the more worthy of remark, in proportion as it approximated to our own epoch. Not to extend, on the present occasion, beyond the limits of botany, the total number of known plants, which, in the catalogues of the earlier authors of the 16th century, did not amount to more than 800 or 900, towards the end of the same century amounted to no less than 2000; in the following century, according to Tournefort, they had advanced to 10,000, in which, however, the varieties are comprehended; when reduced to individual species, properly so called, this number, according to the reckoning of Linnæus, amounted, in his time, to 7000; it was to the extent of 20,000 in the time of M. de Jussieu, and it has quadrupled since his day, and will, accordingly, amount to the number of about 80,000, in the great work which M. De Candolle is now publishing. A single family, that of the *Compositæ*, will contain more than 8000 species in this work; that is to say, will contain more species in a single family, than found a place in the whole vegetable kingdom in the days of Linnæus. The genius of M. de Jussieu is nowhere more conspicuous than in that part of his History which he drew from the materials which he possessed at the epoch at which he wrote. The number of these materials has since increased fourfold, as just stated; and, notwithstanding, there is not any grand principle of the natural order, which does not find its place in his book, and scarcely any of the combinations since established by his successors, of which the germ may not be there discovered. Fontenelle admires, in Tournefort, a classification where more than 1200 new species, which, he adds, *were not expected*, could be introduced, and without injuring its foundations. What, then, would he have said of the arrangement of M. de Jussieu, in which nearly 50,000 species, quite unknown at the time the author wrote, have found their place, and almost everywhere, a place which was previously indicated,—a place which *they were expected* to fill?

The work in which M. de Jussieu propounded his arrangement, the fruit of so many deeply-calculated combinations, was the result of fifteen years' unremitting labour. He commenced its impression in the year 1788, and so matured was it in his mind, that the printing was begun before the manuscript was ready; and the author was never in advance of the printer above two or three leaves. To this we may add, that the first sheets had been printed without those *Notes* placed at the close of the characters of the families, which are, perhaps, the most acute and profound parts of the whole book. M. de Jussieu ordered all these sheets to be destroyed, not recoiling from a resolution which would have been most distressing in a common work; he felt that his would be immortal. The impression, and consequently, the compilation, —for they advanced together,—lasted for fifteen months, and the work was published in the month of July 1789. It opens by that celebrated *Introduction*, in which the author anew developes, and on this occasion, in their true order, those great principles which he had already established in his two memoirs of 1773 and 1774. Here, these principles form a complete body of doctrine. A study of fifteen years could not but have imparted to it lucidity, concatenation, and strength: in it we find that the author, by his reflections, by his experience, and by his deep meditations, ascends to the very highest rules of the art of arrangement, and that he connects this art with a new science,—a science which he himself had created, —the science of characters.

Two facts predominate over every idea which relates to the natural method: the one is the very subordination of characters. By taking assistance, in turns, from reason and experience, M. de Jussieu determined, as we have seen, the importance of organs from their function; and, when this function was unknown, he determined it from their constancy; an ingenious plan, by which a fact in the valuation, which is often impossible, generally difficult, and almost always obscure, at all events, in the present state of botanical knowledge, namely, the *function* of an organ, is skilfully transformed into this other, namely, its *constancy*, a valuation which is always simple, easy, and apparent. The second fundamental fact of the *natural method* is, the adjustment of the characters to the groups. In the *artificial methods*, a character is first selected out from all the others, and the species are then submitted to this character. In the *natural method*, the proceeding is the very reverse; the character is submitted to the species. Systematic authors descend from classes to genera, and from genera to species; they descend from general to particular. But M. de Jussieu completely reverses the proceeding; *he* ascends, as he himself has stated, *from particulars to generals;* and all the difference between the *artificial methods* and the *natural method* consists in this,—that the former subject the species to the genera, and the genera to the classes; the latter, on the contrary, subjects the classes to the genera, and the genera to the species; the former subjects facts to ideas, the latter ideas to facts.

In the new department which the consideration of alliances opened up to science, every step of M. de Jussieu excited the attention of the naturalist; the secret of his power being the route he followed. The example of those families which are conspicuously and remarkably natural, constituted his guide in those which are less so. He saw in those families which, in the eyes of all botanists, are natural, such as the *Gramineæ*, the *Compositæ*, the *Leguminosæ*, *Umbelliferæ*, &c., that the species were grouped conformably to the *ensemble* of their structure; and this was a ray of light to guide his path; every character which, upon application to one of these families broke up the species, required necessarily to be excluded; and hence the first condition of every character was to respect the alliances of the species, grounded upon the totality of their structure. And this calculation of the importance of characters, deduced from their relation to the totality of their structure, is the principle upon which the work of M. de Jussieu is wholly based. The especial object of this work, is the distribution of genera into families. Tournefort had previously arranged the whole of the species into genera; Linnæus had conferred upon these primary genera more regularity and precision; and what remained to be done was, to effect for the groups of a more elevated order,—for the groups which had even been omitted in the systems of Tournefort and Linnæus, in a word, for the *families*, that which these great botanists had done for the genera.

M. de Jussieu distributed all the genera known at the time he wrote, amounting to nearly 2000, into one hundred *families*. He founded each of these primitive families upon a fixed totality of characters, and he demonstrated that the concurrence of character was indispensable; for each character, taken separately, might appertain to many families; it is their combination, then, and a combination different in each, which alone is the peculiar character of each family, and so constitutes it. The *character* of each family, therefore, is not *unique;* nor is it *arbitrary*, as in the artificial system: this character, which is *one*, but collective, or a *multiple*, is the assemblage of the characters furnished by observations, and by facts, as the most constant in each.

It will be at once perceived, that a light so new could not be brought to bear upon all these *families* and principal groups of the vegetable kingdom, without the author reviewing the whole of these elements, more especially the species, the genera, and the character of each genus. Throughout this immense labour M. de Jussieu never relaxed; the quick eye of the naturalist everywhere admires that consummate experience, that happy tact, and that profound sagacity, which probably had never, previous to this time, in any branch of science whatever, been exhibited in an equal degree.

Naturalists, as already remarked, had, at a much earlier date, perceived that certain families of plants fell into a natural arrangement. As early as 1672, Morison had recognised the principal traits of the *Umbelliferæ*. A few years later, Ray attempted a distribution of the vegetable kingdom upon a much more extensive plan; and he pointed out the great division of plants into dicotyledonous and monocotyledonous, and placed the palm in the latter group. Finally, in the year 1689, precisely a century before M. de Jussieu, Magnol published his work upon the *Familles des Plantes.* But neither Magnol, nor Ray, nor Morison, had prosecuted these general views into detail, and, however happy, being scattered and unsupported, they came to nothing.

About the middle of the 18th century, Linnæus, to whom Botany was indebted for its nomenclature, its descriptive language, and its most precise artificial system, by far the most rigorous it ever had, published a series of *Orders*, or of *Natural families*, which amounted, in the year 1738, to sixty-four, and which he afterwards reduced to the number of fifty-eight.

These two attempts, however, presented nothing more than a series of names, without any explication, development, or the slightest indication of the motives which had influenced the author, either in the formation or the classification of his families: it was, as M. de Jussieu remarked, " a kind of problem, which Linnæus left to be solved by his successors," but which never has been solved. A more complete work, and, as it relates to *Natural families*, a much more important one, was that of Adanson, published in the year 1763. That feature in Adanson which strikes us as the most remarkable, is his character as a Reformer. Even in his first work, his *Histoire Naturelle du Sénégal*, this trait is conspicuous, in which, as it regards the classification of *shell-fish*, he altered it from top to bottom, and at once placed it upon its true basis, namely, upon the contained animals,—the shell-fish, of which the shells are, in fact, only the coverings. But his original and renovating genius appeared still more conspicuously in his work upon the *Familles des Plantes*. No man more than Adanson ever endeavoured to free the science from its systematic trammels; no one has more thoroughly demonstrated the radical fault of all artificial systems, viz. their being *partial*, as founded upon a single part, upon a single organ, and that organ arbitrarily selected; no one, in fine, has more distinctly perceived, that the arrangement, to be natural, or, in other words, *complete*, must repose upon the *universality* of its parts. What he failed in perceiving was the subordination of these parts to one another: and the immense influence of prepossession, even in a mind of his caliber, may be seen in the following sentence of Adanson's Report to the Academy upon M. de Jussieu's Memoir. " The principles of M. de Jussieu," says Adanson, in an unedited document preserved in our archives, " will experience opposition on the part of botanists who conceive that, for an arrangement to be natural, it must be founded upon all the parts taken together, without giving an exclusive preference to one over the rest." Adanson's mistake must here be evident to every one. What he objects to in the phrase *exclusive preference*, is precisely the *subordination of parts;* and, by rejecting the subordination of parts or of characters, he thereby also rejects that of groups, at least what is most important in these groups; he only admits *families*, the number of which he extends to fifty-eight; he does not admit *classes*,—he perceives not, that comprehension of all the groups into one another, from the first to the last, from the species even to the kingdom, and that graduated generalization which from species ascends to genera, from genera to families, from families to classes, from classes to a kingdom, and which, under another point of view, that is to say, an abstract one, is the whole of arrangement,—this graduated generalization, we repeat, completely escaped him.

The individual from whom M. de Jussieu reaped most advantage in the compilation of his work, was his uncle Bernard; whilst it is also true that the *Catalogue*, like Linnæus' table of *orders*, was only a series of names. The principles, however, which guided Bernard in his *Catalogue*, both in the formation of families, and the reduction of families into classes, have been faithfully preserved by his nephew, and they are those of which the exposition has been given above,—the subordination of the characters among themselves, and the adjustment of these characters to the groups.

Bernard, therefore, had thus the honour of laying the foundation of the Natural Order, because he caught the principles upon which this order is founded. But, on the one hand, he confined himself to the application of these principles, without at all developing them, and probably without even eliminating the theory; and, on the other hand, even when he applied it, he restricted himself to a series of names. There is nothing in Bernard, either of that philosophy of arrangement which has discovered a new horizon for the natural sciences, or of that matured selection of characters which, differently grouped, supply whole families; and these are truly the two unchangeable claims upon which rest the merits of M. de Jussieu. No one will, we trust, suspect us of wishing to exalt the one of these celebrated men at the expense of the other. Bernard is the discoverer, and he made the first advances on the path: and if his nephew has gone farther than he did, it is because he started from that advanced position to which his uncle had led him. It is the truth only that we here seek, and this solely in the study of their thoughts, and it appears to us, that the peculiar characters of their respective geniuses may here be unravelled and distinguished by distinct traits. Bernard, by the power of an acute perception, recognised the principles of the natural order, but he discovered them without explaining them, and much more for his individual guidance than for the advantage of others. Laurent grouped them, especially when explaining them and making them familiar to others. These principles, if I may so speak, sprung up in the one, and were matured in the other; the one discovered them, the other propounded them; in a word, the one is that earlier age in which genius descried them, and the other that later age in which genius drew deductions from what had been discovered; and there is between M. de Jussieu and his uncle,—between their labours, their methods, and the impress of their thoughts,—all the difference which exists between these two ages.

If, after thus contrasting the work of M. de Jussieu with what had been done previous to its appearance, we compare it with what has followed, its position will remain not less singular. We have seen that the author established one hundred primitive families, and not one of these families has been suppressed, whilst more than the half of them have not undergone the slightest modification. Three of them have been carried over, and entirely, into neighbouring groups; which, however, is nothing more than a different mode of association. The majority of the others, as a natural result of the immense number of new species which have been collected within the last half century, have been necessarily subdivided and separated, but almost the whole of them precisely into those sections and groups which M. de Jussieu himself had indicated. Finally, there have been five, and five only, which have been found only partially to be natural. The errors, therefore, are connected only with some fragments of families, —with some scattered genera; and, even as it regards them, there usually occurs some note, or other indication, some query, which directs the eye towards the truth, and in a way which the most wonderful sagacity only could at the time perceive, so few were the elements which the author then had at his disposal, and so many new ones, it was necessary for him to collect for the complete and satisfactory establishment of the truth.

If it be now inquired what is the peculiar merit attached, I may say, to every page of this work, by which it is so strikingly distinguished from all those which had previously appeared in an investigation so vast, and which had been so often undertaken, it will not be difficult to answer, that this merit consists especially in that continual precision in details, which arranges each part in its true place;—which, not confining itself to the chief results which are rapidly perceived in every genus, neglects none of those truths, in all the orders, upon which the results are based; a merit which is altogether essential in a study in which every one of the facts is necessary,—in which one cannot be supplied by another, and in which all are nearly of equal difficulty of acquisition,—a merit of the rarest character, and which well explains the profound expression of Buffon, that patience, that is to say, constancy in great efforts, is genius.

M. de Jussieu has been censured, and not without reason, on account of the arrangement of some of his classes being based upon the form of the corolla. This is, in truth, the vulnerable part of his Method, and he was himself perfectly conscious of it. "These classes," he remarks, "have this defect, that the allowance of some exceptions is necessary to their existence." He adds, that, viewing the arrangement with the severe glance of science, and not as a matter of convenience, it would be necessary to confine ourselves to the only invariable character, namely, the *lobes of the embryo*, and *the insertion of the stamens*. And, in proportion as the number of species has gradually increased, it has more decidedly been ascertained that this last character, taken from the insertion of the stamens, is the only one which does not vary, and which, therefore, ought not to be excluded in the formation of the character of the classes. In the same way, our continued progress leads also to the establishment of the great division, which is founded upon the lobes of the embryo. M. Desfontaines, by a singularly beautiful discovery in vegetable anatomy, has demonstrated, that the relations drawn from the organs of vegetation, in this division, every where correspond with the alliances drawn from the organs of fructification. We may even say that this striking confirmation, obtained from the structure of the stem, places the first three groups of the vegetable kingdom in quite a distinguished rank, which does not so accurately belong to the common name of *class*, given by M. de Jussieu alike to these three groups, and to the others which immediately succeed. These first three may indeed be compared to the four great FORMS *(embranchements)*, vertebrata, mollusca, &c., which Baron Cuvier established in the animal kingdom, and under which are ranged, at a certain distance, the Classes properly so called ; an arrangement which it will probably be expedient to adopt in both the kingdoms, and to designate by a peculiar and determinate appellation.

The question here occurs, How are we to fill up the interval which separates these three primary groups of the vegetable kingdom from the simple *families*, without admitting between these groups and the families any thing that is arbitrary or artificial? And here, again, M. de Jussieu has the merit of having traced the method of association, frequently indicated in his work, between the several families; a character which was clearly seen, and distinctly expressed, by Mr Robert Brown. " The real problem," says this excellent botanist, " is to combine the families into more considerable and equally natural groups." And this problem has, in truth, been already solved in a certain number of cases by Mr Brown himself, and, when solved in the whole, it will yield the only valuable general classification.

When M. de Jussieu published his work, he was, without dispute, the first naturalist of his day. Nevertheless, we are not to imagine that this work had, from the commencement, all the reputation which it has subsequently acquired. It appeared in the year 1789, in the midst of that great revolution, which opened to France the portals of its new destiny, and hence it was scarcely to be expected, that a revolution in the humble science of botany would attract very peculiar attention. This work, moreover, was too much in advance of the currently received ideas, to be comprehended without long and severe study. It was, therefore, only gradually that the principles of M. de Jussieu made their way among naturalists, and more especially foreign ones.

As soon as the new social order established in France permitted the return to quiet and peaceable studies, a circumstance occurred which suddenly communicated to these principles a new impulse, and an unlooked for influence. A young naturalist, who had hitherto been hid in a provincial town, and whose discovery, for it was one, and one which has been disputed among our contemporaries, and in which M. de Jussieu had unquestionably the honour of a share, published two memoirs, in the year 1795, the one upon "*The Principles of the Classification of the Mammalia*," and the other upon "*Linnæus' Class of Worms*." These two memoirs were to Zoology, what M. de Jussieu's first two memoirs had previously been to Botany; they completely changed the features of the science; and from that period, in zoology, as in botany, the words *natural arrangement* had their full meaning, and *natural arrangement* was *the arrangement founded upon organization.*

Cuvier bestowed, long afterwards, and upon a solemn occasion, a noble tribute upon M. de Jussieu. He distinctly declared in his *Historical Report upon the progress of the Natural Sciences since the year* 1789, "That the work of M. de Jussieu constituted *an epoch probably as important in the sciences of observation*, as the chemistry of Lavoisier did in experimental science." I am not, however, sure, that this other tribute which the Baron paid him in the first of the memoirs above alluded to, is not still more remarkable. "Zoologists," he remarked, "had no idea of that calculation of characters, of which botanists had clearly recognised the reality, and which one of them has so admirably developed in a work, of which all the other branches of natural history will speedily feel the happy influence, although it refers to one of them alone." Here we perceive the philosophic chain of advancing science is reknit by new links; and the efforts of the young Cuvier, for the renovation of zoology, are attached to the work, which had so much improved botany.

But zoology presents for the application of the *natural method*, and particularly for the application of the natural arrangement founded upon reason, a field much more extensive than does botany. In animals, the organs are much more distinct, the functions more marked, and, consequently, the subordination of characters more evident. The modifications of the external organs in them, *in a conspicuous manner, depend* upon the modifications of the internal organs; the brain, the heart, and the lungs, for example, cannot change without the other parts, necessarily correlative to these, also changing; the reason of this strict concordance between all the modifications of the animal economy is palpable, for the principle of the *subordination of organs* becomes in them the very principle of the *conditions of the existence of beings.* But, in addition, the science of characters, by its application to zoology, has assumed a wider and more elevated bearing. Arrangement has perfected itself, by becoming more general, and by extending itself from one of the kingdoms of organized nature to another; and this by the labours of two authors, who, compared together, present very distinct traces, whereby the whole is perfected. Jussieu pursued a protracted chain of details, with unwearied patience and indefatigable sagacity. Cuvier, by rapid strides, reached the ultimate consequences, overleaping what was intermediate; it was the character of the one never to be disheartened in experimental observation, the only one, in fact, which is applicable to botany; and of the other, to seize, at a glance, the natural method, which best suits zoology; together, they supplied a new logical energy to human thought, the energy of *arrangement,* which, consisting in the union of objects according to their common qualities, is truly to the science of observation, what *analysis,* or the act of decomposing them into their distinct elements, is to the experimental sciences. And in the same way as analysis, originating from the experiments of Galileo, has gradually passed from the physical sciences into the more general science of the understanding *(entendement),* thereby becoming the *philosophical analysis* of Condillac, so the *arrangement* or method produced by the researches of modern naturalists, is about to produce similar effects upon the abstract study of philosophy; and it is only after this has been effected, that general philosophy,—the result, not less of *the art of classifying ideas,* which has hitherto been neglected, than of *the art of decom-*

posing them, which has long been sedulously prosecuted,—will be perfected.

M. de Jussieu published his work in the year 1789. Almost always shut up in his cabinet during years of unceasing labour, he had remained a stranger to the political movements which were then agitating the whole nation. The last sheets were scarcely terminated when he found himself appointed, in one of the departments, to the mayoralty of Paris. This mayoralty, formerly considered as but one office, was now divided, as is well known, into many departments, and that of the superintendence of the hospitals fell to M. de Jussieu. It was upon this occasion that he published his *Reports upon the Hospitals of Paris*;—a species of labour highly calculated to render science respectable; and in which the author had been hitherto preceded only by one member of this Academy, whose remembrance will never cease to be admired by mankind, I mean Bailly, alike illustrious and unfortunate.

In the year 1793, the *Jardin des Plantes* received a new organization, and took the title of the *Museum d'Histoire Naturelle*. Daubenton was its first director, and M. de Jussieu succeeded him. During these difficult times, Jussieu wholly devoted himself to the administration of this superb establishment, to which were closely allied the celebrity of his name, and his family recollections.

On the formation of the *Institute*, he naturally became a member. He was one of the first Presidents of the New Academy of Sciences; and he was Vice-President of the year which was distinguished by having Napoleon for President.

In the year 1804, the chair of the Materia Medica of the Medical Faculty having become vacant by the death of Peyrilhe, Jussieu offered himself as a candidate, and all other applicants immediately retired. On becoming professor, he took, as the foundation of his lectures, the fertile principle of the agreement of the properties of plants with their botanical affinities; a principle which he had pointed out at an early period of his career; and a new application of the natural method, perhaps of all others the most proper to extend the domain of Materia Medica. In the year 1808 he was appointed a member of the Council of the University.

During the concluding half of his life, the most fixed purpose of M. de Jussieu was to bring out a new edition of his great work. Unfortunately, his bodily energies naturally diminished in proportion as the materials of the science augmented; he overtook only a small part of this magnificent work, but the fragments even exhibit an uncommon profundity which would have established the reputation of any other individual. These fragments form a series of memoirs, which, from the year 1804 to 1820, and almost uninterruptedly, were inserted in the *Annales du Muséum.* More than a half of the author's primitive families are there reviewed; each of them is examined in detail, and in each the genera of which it is composed. In the year 1798, M. de Jussieu had not been able to avail himself of the great work of Gærtner upon *Fruits.* On this occasion, he made it the term of comparison, and the touchstone of all the new alliances which suggested themselves. In studying the different kinds of corn, Gærtner had thrown the light of anatomy upon that very organ from which M. de Jussieu had derived the chief foundation of his arrangement. When applied to the science of relations, the observations of M. Gærtner assumed a new and unexpected importance; M. de Jussieu employed it to shed new light upon the calculation of characters, upon the formation of families, and on that art, so little known previously in botany, of applying the one to the other of those mainsprings and resources, viz. anatomy and arrangement, on which alone was, henceforward, to depend the future progress of the science.

M. de Jussieu relaxed from his more severe labour by publishing some productions of another kind, but still having natural history, or, what expresses the same idea in different words, the *Jardin des Plantes,* for its object. I here allude to his *Mémoires sur le Museum.* The *Jardin Royal,* originally founded under Louis XIII. by an edict of the year 1626, was at first nothing more than a *garden for medical plants.* This was at the time its legal title; and its museum was nothing more than a useful *drug-shop.* M. de Jussieu dwells upon the insignificant commencement of this drug-shop, which has since become the most magnificent establishment devoted to the study of nature that is to be found in the world. He recounts the difficulties of every kind which it had at first to overcome, not forgetting the hostility it encountered from the *Faculté de Medecine*, who especially opposed *Chemistry*, the subject of one of the new chairs connected with the museum. They contended *that it should not be taught in Paris for various weighty causes and considerations, and because it was prohibited and censured by act of Parliament.* M. de Jussieu also brings within our notice those illustrious men to whom this beautiful establishment chiefly owes its splendour, as Tournefort, Duverney, Bernard de Jussieu, Vicq.-d'Azyr, and Buffon. He stopped at the great epoch of Buffon; and it is a cause of sincere regret that he did not advance to the succeeding one, which probably was in no degree inferior. In this new epoch, Haüy, unveiling the mechanism of the formation of crystals, subjected even the phenomena of nature to the laws of calculation; M. de Jussieu subjected to laws of another character, namely, to the laws of reasoning grounded upon observation, the new existences which were accumulated from nearly every quarter of the globe with a profusion heretofore unexampled; and Cuvier, penetrating into the bowels of the earth, discovered those generations which had been long extinct. He created an art, by which he allied and united the scattered fragments of these extinct generations, and gave them, by the laws of comparative anatomy alone, a new existence, and, as it were, a new life.

Nor would I forget any of the writings which came from M. de Jussieu's pen. I find in his *Thesis*, published in the year 1770, the first distinct ideas upon those multiplied analogies of vegetables and animals, and upon the unity, if I may so call it, of the two organic kingdoms; views which were then nearly new, for they had been indicated previously only by Pallas; views as profound as they were new, and which have since been so brilliantly developed by Vicq-d'Azyr and by Cuvier. The only production of M. de Jussieu which might be omitted, and which perhaps ought to be, since it is foreign to the subject of natural history, is his *Rapport sur le Magnétisme Animal*, which was published in 1784. Nothing in this production is associated with those profound and practical questions which were the habitual subjects of the great naturalist; and consequently, we need not hesitate to avow, that nothing is here to be found which bears the impress of the firm and judicious mind of the legislator of botany.

At the period of the Restoration, M. de Jussieu was one of the council of the University, and of the *Ecole de Médecine.* In the year 1815, the Council of the University was replaced by that of *L'Instruction Publique,* and M. de Jussieu was not appointed to this council. In the year 1822, he was excluded from *L'Ecole de Médecine,* along with Vauquelin, Chaussier, Pinel, Deyeux, Genettes, &c. In 1830, when the injustice might otherwise have been repaired, most of these celebrated men were dead; and M. de Jussieu himself, now eighty-two years of age, was too old to resume his place in the faculty. In the year 1826, he resigned his chair in the museum, in favour of his son M. Adrian de Jussieu; and some years afterwards, viz. in 1831, he had the happiness of seeing his son become a member of the Academy.

Labour, throughout his whole life, had ever been a delightful and necessary exercise to M. de Jussieu; and the whole of his time not occupied by his public duties was spent in his study, meditating upon science and arranging his plants. He used even to read in the streets. He was always very near-sighted, a conformation for which his family has long been remarkable, and, when yet far from being an aged man, he entirely lost the use of one of his eyes, and, towards the close of life, his sight was so weak, that he could neither write nor examine any minute object. At this time, being no longer able to work himself, he obtained the services of those who gave him an account of the works of others; and all those delicate attentions he had so long been in the habit of rendering to his uncle Bernard after he became blind, were now repaid to himself by one who was still more dear. Problems were prepared to him which occupied his mind, constituted like Bernard's, for meditation and combination. His information was kept full to the level of the most recent discoveries; and if, among these discoveries, any thing bore upon his ideas of characters and arrangement, his botanic instinct, which was ever active, instantly seized upon it; every thing was promptly reduced to its most simple expression. M. de Jussieu then digested these new results with singular elegance in the Latin tongue; and, preparing a second edition of the *Introduction* of his great work, he was satisfied only when he had placed it there in its appropriate place.

There has very lately been published, in the *Annales des Sciences Naturelles*, a memoir by M. de Jussieu. It is his last, and the production of an old man of nearly ninety. In it we are astonished to perceive to what an advanced age the author preserves all the distinct accuracy of his mind; and still more, to find with what power he treats those ideas which were first promulgated in the year 1773, then again in 1774, and in 1789, and constantly rehandled since, till they occupied his thoughts at the latest hour. And in the midst of all this he in no degree deceived himself. He often repeated, that he thus worked only from choice and habit, and not for the instruction of others. On one occasion he very good-humouredly expressed to his secretary his reasons for writing in Latin rather than in French. First of all, said he, it occupies somewhat more of my time, and that is so much gained; and secondly, things, which in themselves are very common-place, when expressed in a foreign tongue, assume a less vulgar physiognomy. Were I to express my ideas in my mother tongue, I should esteem them worth nothing, and could not work them out.

M. de Jussieu lived personally to enjoy a large portion of his high reputation, of which he never ceased to assign the chief part to his uncle; and this modest apprehension of his own merit found utterance in an expression which I may here quote. Some one was complimenting his son in his presence for bearing so illustrious a name: Assuredly, said M. de Jussieu, the name has been of vast advantage to me.

Until his last declining years he never failed, when in Paris, to attend the meetings of the Academy; and even when he lost his sight, and afterwards his hearing too, he still continued his attendance. He delighted in the conviction, that he was among his brother associates. He was for sixty-three years a member of this learned body, and for sixty-six years a professor of the *Jardin des Plantes*. In the country, where, towards the end of his life, he passed a portion of the year, his principal pleasure was walking. He still sought for plants; and though he was almost blind, as just stated, yet he closely examined them till he recognised them. When his eyes completely failed he endeavoured to distinguish them by the touch, and a discovery so accomplished was regarded as a triumph, for it was a kind of problem, or an enigma, in fact, a difficulty which was overcome. This characteristic may be clearly recognised in the following words, which I borrow from one of his first memoirs, and which give utterance to sentiments which may be most appropriately introduced toward the close of this Eloge, since the author, in seeking to define, in his own manner, the qualities of a great botanist, seems to have drawn a portraiture of himself: " Any man of genius," says M. de Jussieu, " may make systems, and may vary them to infinity; but the natural order will never be the work of any other than a consummate botanist, whose patience in examining the minutest details equals his genius to deduce consequences, to draw conclusions, and, in a word, to make botany a science, not of memory and nomenclature, but a new science, which possesses its combinations and affinities like chemistry, and its problems like geometry."

The character of M. de Jussieu was developed very early in life, and he most steadily maintained it. The severe habits of Bernard had given it a precocious maturity. When still very young, M. de Jussieu received from all those who surrounded him, and many of them much his seniors, the warmest esteem, mingled with respect. Like his uncle Bernard, he was very pious and devout. Though a man of superior genius, and a philosopher of extraordinary celebrity, he knew the secret of maintaining a peaceable career; and he found this secret, chiefly, in the equanimity of his mind. He allowed himself to be attacked, in nearly all languages, without responding. He used to remark, that, if he were wrong, it was very right he should be attacked; and, if right, all attacks would prove harmless.

M. de Jussieu was twice married, first in the year 1779, and again in 1791. He had two daughters by the former marriage, and one daughter and a son by the second, a son known to us all, M. Adrian de Jussieu, member of this Academy.

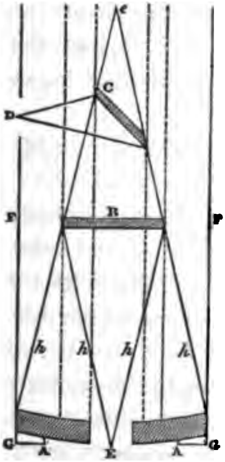
By a remarkable contrast, in the midst of the many resemblances which he had to his uncle, M. de Jussieu loved the sweets of society, as Bernard did those of solitude. His society, it is true, was limited almost to his own family, which, however, was very numerous. Besides those already named, he had almost adopted two nephews, and a niece who subsequently became his daughter-in-law. By every member of this family M. de Jussieu was quite adored. His second wife and his eldest daughter especially, tended him with religious and affectionate care. In this all the other members participated, or, at all events, wished to participate. On his part, again, M. de Jussieu had an exhaustless affection for all his relatives. He took particular delight in gathering his grandchildren round him, in witnessing their amusements, and amusing himself with them; and he found that his library, at least, possessed this virtue, that the figures of flowers and animals with which it abounded often kept them in his company for whole hours. He loved young people greatly; and, having enjoyed the privilege of long life, he was no stranger to the drawbacks which are attached to this privilege. He lost one by one the majority of his early friends; in the same proportion new generations supplied him with others; and he died surrounded by young botanists, whose affection charmed him not less than their respect.

M. de Jussieu became much bent down with increasing years, but naturally he was very tall. His constitution was robust. To his taste for bodily exercise; to his habits of active mental employment, and which he continued till the last; and to the assiduous care of every sort with which he was surrounded, he appeared to owe his excellent health, which was interrupted only at the close of his life, and then only by some slight indispositions. His fatal malady began with slight indisposition; but speedily, from the obstinate and entire defect of action of the digestive functions, the worst was apprehended, and he died on the 17th day of September 1836, aged 88 years, 5 months, and 5 days.

For nearly half a century, during which time his great work has been published, Jussieu's superiority has been undisputed by any one; and he saw all the botanists around him labouring in perfecting his System. Desfontaines confirmed it by his beautiful observations upon the structure of stems; Petit-Thouars also applied it with singular sagacity, as did Richard, the father of precise and detailed vegetable analysis, whose austere style is well known, and who, nevertheless, distinguishes the author of this arrangement *as the first botanist of Europe;* whilst all the celebrated botanists who have appeared within the last half century, have proclaimed him their master. It has been given to few individuals to exercise such an influence over other men; and to still fewer to be themselves witness of it: it is a career which is probably unique, belonging equally to the 18th and the 19th century, and which, in time as in honour, is allied with the two greatest events connected with the natural sciences in these two remarkable centuries, with the *Chemistry* of Lavoisier, which was published in the year 1789, the same year with M. de Jussieu's work, so closing the 18th century; and with the *Recherches sur les Ossemens Fossiles,* by George Cuvier, which signalized the commencement of the 19th century.

On the comparative merits of the Reflecting Microscope of Sir David Brewster, and the Catadioptric Engiscope of Professor Amici of Modena; with an account of a new Reflecting Telescope for Terrestrial Objects. By CHARLES R. GORING, M. D. Communicated by the Author.

SIR DAVID BREWSTER, whose various ingenious writings on the microscope need no feeble testimony of mine in their favour, has, in his Encyclopædia Britannica treatise on the microscope, mentioned a new construction for reflecting instruments, which he seems to consider far superior to any other hitherto invented. I am disposed to think, from a consideration of the drawing he has given of it, that *he cannot have been at the trouble and expense of trying it well and fairly,* else he would have found out the necessity of making the small plane metal *of its proper size, viz. at least one-half of the diameter of the concave one.* No less can it be if the whole aperture of the concave one is to be called into operation. In the wood-engraving in Sir David's treatise it is made much in the same proportion as in an ordinary reflecting telescope, and looks very well. Making improvements on optical instruments *on paper* is a delightful amusement; but it is something like raising troops and revenues in the same way. However, when the drawings *are truly made upon rather a large scale*, it cannot be denied that much may be demonstrated by them, so that the cost and trouble of executing things, which cannot possibly turn out to be of any real utility in a practical point of view, may thus frequently be saved. I have appended to this paper a tolerably true drawing of a metal of 27½° of *acting aperture;* it may be supposed to be any focal length, for example six-tenths of an inch. The Brewsterian and Amician constructions are both shewn in it, and speak for themselves as to the portion of the concave metal which their plane specula respectively obliterate and render inert. A A, is the concave; B, Sir David's plane mirror; C, Amici's, (*both made of the smallest size possible, their extreme edges being in full operation,* and the object supposed to be close to the back of the concave, in the one case at E; and to the side of the tube in the other at D;) F F, are sections of the sides of the tube; G G, sections of the piece of brass applied to the back of the concave to fasten it in its place; *e*, is the focus of the concave, supposed not to have been reflected back by the plane B to E, or by the diagonal C to D; the four *h's* are dotted lines to express the alignments of the diagonal metal and the large plane with the common elliptical one.



It will be found by admeasurement that the large plane blots out *one-half* of the centrical and finest part of the concave, and, consequently, one-quarter of the whole of its light! The diagonal little more than one-quarter of the centrical portion of metal, and, consequently, not much more than one-sixteenth of the light. This is according to the drawing, which is made *exactly as fair for one construction as for the other.* In practice, however, it will be found that as it is necessary to cause the pencil to be projected outwards considerably more than in the said drawing *for the purpose of shewing opaque objects, both* *the plane and the diagonal must be made larger than they are represented,* and likewise placed nearer to the concave, which, of course, will cause a still larger hiatus or blot in the centre in both instruments.

2dly, As the diagonal and plane are both nearer to the eye-piece than the concave, the visual pencil, which consists of the image formed by the eye-glass of the concave with a dark spot in the centre, formed by the small metal, will in both cases have a larger blot in it than is in the ratio of the size of the small metal to the large one. Owing to these circumstances, in the Amician form it is but just possible to make the image of the diagonal one-third of the diameter of the concave, whereas in the drawing it is only a little more than one-fourth. The large plane being nearer to the concave than the diagonal, will not cause an augmentation of the size of the blot, in precisely the same ratio that the diagonal does; nevertheless, the size of the said black speck in the visual pencil will be larger than the proportionate size of the metals, even when adapted for viewing opaque objects, will indicate.

3dly, There is another circumstance to be considered, highly unfavourable to the Brewsterian construction. It will be remarked that in the Amician form, the pencil of rays from the object will only have to pass through the side of the tube, *which is very thin,* at D, whereas in Sir David Brewster's it must pass through the concave metal, which I have drawn *no thicker in proportion than in a reflecting telescope;* still it will be much thicker than the tube, but *I do not think that the concave can be so executed.* Mr Cuthbert always makes his metals *very thick,* and also solders a piece of brass at the back of them, in order to make sure of his centering and adjustment, and also to form a handle by which he may hold them comfortably, and *feel what he was at when figuring them.* Imagine a metal of three-tenths of an inch focus, and two-tenths of aperture, made as thin in proportion as that of a telescope! How can it be worked? People have no idea of the delicacy of these things, or how easily the figure of a metal of *ordinary thickness* is destroyed. Let us suppose the artist stuck it with cement to a handle to enable him to turn and work it, and had the luck afterwards to detach it without breaking it; it is as likely as not that if its figure was perfect while he was working it *attached to the handle, it would undergo a change when detached from it*,* and he would be compelled to make it of considerable thickness at last, to insure the stability of its figure, and its adjustment when fixed in the tube.

Reflecting instruments are proportionally much worse in their performance than refractors, if in the least out of adjustment; and the adjustment of very small metals to each other is an operation of almost inconceivable delicacy. For this purpose, I repeat, as well as to form a handle, the piece of brass is soldered to their back, and the metal and its support, which has a screw behind to fix it to the chuck, turned very true together, at the same chucking in the lathe in which the figure of the metal also is roughly formed, so that the centering of the metal may be perfectly in unison with its curve or figure, and likewise with the tube. Thus, in the metal mentioned, three-tenths focus, and two-tenths aperture, the thickness of the metal and support at the back is not usually less than two-tenths of an inch. Of all these difficulties in execution, a mere theorist in the closet must of course be supposed ignorant; but *if* it is absolutely necessary to make these small metals very thick, and I think it will be found so, how can the two metals in Sir David Brewster's construction be sufficiently approximated to each other, and the size of the plane increased in the requisite proportion necessary to enable the instrument to act on an opaque object with its full aperture, *without reducing the visual pencil to a mere ring of light, producing, in consequence, a strong nebulosity in the middle of the field of view, and making the instrument as dark as a wolf's mouth.*

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* The late Mr William Tulley told me the following anecdotes of metals:—He had polished a Gregorian metal of five inches aperture in the evening, cleaned it, and placed it in its tube. He then allowed it to stand in the open air till he thought it had acquired the temperature of the atmosphere, and on trying it, was perfectly delighted with its performance. He left it out, and the next morning he got up before daylight to try it again, *and found it one of the poorest things imaginable!* By that time it had *indeed* come to the temperature of the air, and consequently changed its figure which it had not, it seems, in the night before. Sir J. Herschell once told me, if I remember rightly, that his large telescopes required fully four hours exposure to come to the temperature of the air; they then perform as well s small ones.

Mr Tulley likewise related to me that he was once engaged a whole week in figuring a metal of three inches aperture and six focus for a dumpy Gregorian; when he had succeeded after infinite trouble, some one burst suddenly into the room where he had been employed on it, *violently kicking the door open.* The metal, which was of the best composition and *well annealed*, instantly broke into two pieces, apparently from the concussion produced in the atmosphere of the room, just as windows and mirrors are cracked by the fire of artillery.

Mr Charles Tulley observed to me that one day he was viewing the nails on the dome of St Paul's with a Newtonian telescope *placed in the sun*, and the diagonal metal of which was about one-eighth of an inch thick, soldered to a piece of tube, *without any support at the back of the speculum*; he found, though the figure of the plane was good, that it shewed the said nails oval (their heads are round), and distorted the figures of other thin proportionally. The rarefaction of the air in the tube of the telescope had been sufficient to alter the figure of the plane mirror which the rays of the sun could not get at; had it been made of a solid lump of metal, the effect, Mr Tulley said, would not have been produced.

The metal of three-tenths focus, and two-tenths aperture in the Amician form, acts perfectly well upon opaque objects, either with or without a small silver cup. I do not believe that an opaque object will ever be shewn at all by a similar speculum in Sir David Brewster's.

4thly, It must be evident, that a tube presented sideways towards an object, will allow the light to fall on an opaque body placed very near to it; whereas, the back of a metal of the same diameter with the tube, placed at the same distance from the object, will completely shade it.

5thly, A plane metal reflecting at right angles as in the Brewsterian form, can by no means reverberate so much light as one at an angle of 45°, and it is this circumstance which gives the superiority to the Newtonian telescope over the Cassegrainian and Gregorian forms.

In a note appended to Sir D. Brewster's critique on my solar camera microscope, he is pleased to quiz me rather unmercifully about my ignorance of the fact, that TOTAL REFLECTION takes place at an angle of 41° 49′ from the surfaces of glass! We must all live and learn. I shall proceed, therefore, to turn this grand and capital discovery to account. First, then, I shall substitute a bit of plane glass for the diagonal metal of the Amician, (which latter certainly does by no means, any more than a *silvered* rectangular prism, reflect all the light which falls upon it). TOTAL REFLECTION will then take place from the diagonal; now, then, is Sir David's instrument beaten hollow, as I suppose he will admit. His double achromatic prism for the Newtonian telescope, and that for the Amician engiscope, to refract the rays outside of the tube, instead of using a plane diagonal, must be laid on the shelf; a bit of plane flint-glass is a far better thing; from the Newtonian telescope down to a camera obscura, let glass be substituted for diagonal metals, looking-glass, and rectangular prisms silvered on the reflecting side,—let the Herschelian telescope be abandoned for the Newtonian, with a glass diagonal, which will be just as luminous; for the head of the observer in the former will intercept as much light as the plane piece of glass.

Lastly, if people are not satisfied of the validity of my objections to Sir David's instrument,—I say *let it be executed*, and tried against an Amician.

Sir David has recommended the combination of metals, whose merits I have just discussed, to form a solar reflecting microscope, for which purpose, the principle is still less adapted than for an engiscope. He, moreover, seems to think a perforated metal with an opaque object in its focus, illuminated through the hole behind it (which was invented by Mr C. Tulley *v.* "*Micrographia*," p. 40), will make a most capital reflecting solar for opaque objects, but I can only say it never did so in my hands.*

It is much to be regretted, that Sir D. Brewster has not favoured us with an account of the celebrated solar microscope, which *I have been told*, was executed under his direction, I think, for the University of Edinburgh, and which gave such universal satisfaction. This is surely a great sin of omission in his treatise on the microscope, where I had fully expected to have met with it. I cannot help thinking that, when Sir J. Newton invented his telescope, the idea of causing the cone of rays to be returned back by a plane speculum to a hole in the centre of the concave metal must have presented itself to his mind, or to any man's who thought on the subject, especially as the Gregorian telescope had, I think, been invented previously; but he, no doubt, saw instantly (as any one else must), how far preferable it would be to cause the cone to be directed to the side of the tube instead, on account of the small size the diagonal required to effect that purpose, and the greater quantity of light it would reflect. Amici reversed Sir I.'s construction, and invented the best reflecting engiscope we shall ever possess, and the only one that has ever come into use, or is able to compete with the refractors now made. Now Sir D. B.'s construction for a microscope may evidently be reversed, and converted into a telescope *of small size* for terrestrial objects; and it appears to me that such an instrument will be the best application of it, or, at any rate, it will be far better in proportion than the microscope.

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* This seemed to arise from the illumination being *too direct and giving no shades*; for the same reason, I suppose, I never succeeded in getting a good image of an opaque object in the solar from a lens set in a silver cup; *the illumination must be oblique, or all is confusion.*

Receipt to make a small dumpy Reflecting Telescope, for terrestrial objects, à la Goring.—Work a metal of 3 inches aperture to a parabolic curve for a focus of 9 inches; it must have a hole in its centre ⅝ths of an inch in diameter. Then figure a plane one ⅞th of an inch in diameter,—it might be made larger in the first instance, and then have its bad edges turned off with a diamond tool; it will also be advisable to turn a shoulder ⅛th of an inch wide all round this speculum in addition, so as to leave its bright surface only ⅝ths of an inch in diameter (which is all that comes into operation). The shoulder is to be blacked afterwards, that it may form a black circlet,—the image of which, on the visual pencil, is to co-operate with an eyehole in excluding false light. Both metals are then to be fitted up in a tube about 8 inches long, with the same adjustments employed in Cassegrain's, as the focus of the instrument will be adjusted in the same manner. When the telescope is used to view a distant object, the surfaces of the two metals will be 7⅛ inches distant from each other. In the next place, a slender erecting eye-piece must be made like those used for the smallest spy-glasses, but having its objective part, or that which forms the secondary image, composed of two double cemented achromatics *in contact*, having ⅜ths of an inch of aperture, and about an inch of solar focus,—one triple *cemented* achromatic (like those made by Mr Pritchard), would also do *if very perfect.** The total length of this eyepiece will be about 6 inches, and it should be so contrived, that its anterior conjugate focus should fall at 1¼ inch in front of the achromatic glass or glasses. The diameter of the tube of this eyepiece must not exceed $\frac{6}{8}$ inch, but its objective end must, *for the last* 1¼ *inch*, be tapered, so that the diameter, at the termination *where the achromatics are*, shall be only half an inch. I had nearly forgotten to mention, that a stop of the same diameter as the image of the large metal formed by the achromatics, (about $\frac{3}{8}$ths of an inch in aperture probably), must be placed in the inside of the tube behind the achromatics, and in their focus, for the usual purpose of keeping out false light. The exterior of the said tube must be carefully blacked, and it might not be amiss to cut the whole of its surface into a screw with a fine tool, to arrest and absorb light more effectually. The power of the instrument will be determined by the depth of the Huyghenian eyeglasses attached to the eyepiece, which must have eyeholes like those of Gregorian telescopes, *very accurately adjusted* to the size and locality of the visual pencil; a mark should be made on the side of the tube which holds them, and a corresponding one on the exterior one, in order that they may be always inserted exactly in the same place, lest any inaccuracy of turning should cause the eyehole to shift a little, and so let in false light. The power employed must not, I think, be less than thirty times, that is, the visual pencil must not be larger than one-tenth of an inch, (which will cause the blot in its centre to be somewhat less than one-thirtieth of an inch), otherwise, the said blot will be apt to cause a faint luminous spot in the middle of the field of view; the power may range from 30 to 60 to suit different states of the atmosphere, and near or distant objects,—(on the former, a higher power may generally be used than on the latter, because there will be less air to look through). It will be necessary to attend to the following circumstances in attaching the eyepiece to the instrument. I suppose the concave metal to be enclosed in a frame which is to be fitted into the tube with a *bayonet catch, that it may always go in exactly into the same relative position;* to the back of this frame is to be soldered a tube made truly centrical with it, to receive the eye piece, which is to be passed into it, so as to *project four inches beyond the surface of the parabolic metal into the body of the telescope;* it must be always inserted exactly in the same place relative to its containing tube, and a mark must be made on each which must be made to correspond (as in the case of the eye glasses), *for fear of deranging the eye-hole*, which will always be delicate and ticklish in its operation in this telescope, being only guarded by the image of the narrow black circlet which encompasses the small metal. If the said circlet should after all be found inefficient, it may be made one-sixteenth of an inch wider, so as to make the entire diameter of the small metal one inch.

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* The said achromatics will be absolutely indispensable on account of the large angle of aperture of the eyepiece (about 20°); it would have been useless to have attempted the construction of a telescope of this kind without them.

Now, let us observe the manner in which the primary image, formed by the concave speculum, is reached by the eyepiece. The small metal, the surface of which, placed at 1⅞ inch distance from the end of the cone of rays, reverberated by the concave speculum, reflects its image to within 5¼ inches distance from the hole in it ;—the tube of the eyepiece projects 4 inches, and its focus is 1¼ inch, $1\frac{7}{8} + 1\frac{7}{8} + 4 + 1\frac{1}{4} = 9$, the whole distance to be got over.

Now, if I am asked whether a Gregorian of the same power and aperture would not be superior to this instrument? I answer, that it would be superior as to light with any given power, in the ratio of what is stopped by the achromatic glass or glasses; it will also be without any defects in performance which may be occasioned by it or them. Still, however, I consider that the instrument just described *will have abundance of light with all the power I propose to apply to it*, and that the eye will never be able to appreciate any difference in performance on *terrestrial objects* produced by the achromatics (if well executed). But, in compensation, the Gregorian construction labours under two great disadvantages; the first is, *it can only be worked perfect to one power*, for immediately you alter this one power (no matter how), the instrument begins to aberrate. The second is, that *when made dumpy*, the difficulty of working its concave to a perfect hyperbolic figure is extreme, and, *when combined with a perfect polish*, taxes human skill to the very uttermost;—the parabola in its curve or contour comes nearer to the sphere than the hyperbola, it is therefore more easily hit; let any workman try which will be figured first, an hyperbolic metal of 3 inches aperture, and 6 inches focus, or a parabolic one of the same diameter and 9 inches focus, *yet both these metals will make a telescope of equal dumpiness*, (supposing the Gregorian to have its small metal of 1 inch focus), for the metals will be at much the same distance apart. I shall now proceed to shew how an excellent trick may be performed with the construction I have recommended, *which cannot be played with a Gregorian, because its concave metal is not parabolic.* Take out the erecting eyepiece and close up the hole with a plug, remove the round plane, and substitute a diagonal one with the necessary adjustment to the side of the tube, and there is a little Newtonian sweeper, which will beat any Gregorian of the same aperture on celestial objects. I conceive that both of these constructions will be equally good with any power they will bear. The Newtonian, perhaps, will carry 120; of course, the erecting eyepiece may be applied to it also, if convenient. I have subjected this instrument to a species of trial, by making an exact drawing of it of the full size, from which the dimensions I have given have been taken.* *On paper*, it seems feasible enough to me, yet I am well aware, that some unforeseen circumstance may arise in the execution which may utterly ruin its performance.

I must protest against Sir D. Brewster's treatise on the microscope, in the Encyclopædia Britannica, as containing any accurate account of my writings and doings where it professes to do so. I select the following instance as an example.

In the very first page of the treatise, it is represented in a manner which no one, I think, can mistake, that I want to quash the word "Microscope" *altogether*, and substitute for it the term "Engiscope." Nothing can be more incorrect, as any one may see by consulting the end of that chapter of the "Microscopic Illustrations," in which I have discussed the point: "*Whether there is a best possible way of constructing the mounting, &c. of microscopes.*" What I really want to do is, "*to apply the term 'Engiscope' to those instruments which shew an image of the object under consideration (as telescopes do), instead of the object itself,*" in order to avoid confusion. *The nomenclature of every science must improve as the science itself does;*—we have new terms daily invented in anatomy, botany, chemistry, geology, mineralogy, &c. &c., which are extremely indispensable to define exactly what we talk or write about. What prodigious alterations have been made in the language of chemistry, for example, in the last century! in another perhaps the terms even now used, may have gone out of fashion. Poor microscopic science has been nearly as much advanced in proportion of late as any other, and the terms I am desirous of introducing into it are the following:—

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* I do not think it worth while to give this drawing on a small scale, because it is so easy to imagine it. We have only to make the convex metal of a Cassegrain plane, and to thrust a small erecting eyepiece through the hole in the concave metal far enough in to meet the image, *and there it is.*

Lens or magnifiers for an unset glass, or one only framed so as to be held in the hand; if two or three are combined, I call the combination a *compound magnifier*, &c.

If the said magnifier is fitted up with a stand, &c., I call it a "*Microscope,*"—a *Single* or *Simple* one, if the lens is single; if two or three are combined, then a "*Compound Microscope;* if the composition is achromatic, then an "*Achromatic** *Microscope,*" or a "*Compound Achromatic Microscope,*" if two or three achromatics are combined. I have applied the same terms to the solar microscope *according to the nature of the construction of its optical part;*—thus, one with a single lens I term a "*Single or Simple Solar Microscope;*" if a single achromatic lens is used to form the image, "*a Solar Achromatic;*" if more than one is employed, a "*Compound Solar Achromatic,*" &c. &c.; and IF A BODY AND EYEGLASSES ARE ADDED TO IT, then I term it a "SOLAR ENGISCOPE," for distinction's sake, just as in the cases already adduced; which seems to me quite consistent. I have described a SOLAR ENGISCOPE forming an image at the bottom of a sort of camera in the "*Micrographia*," and which, BY REMOVING THE BODY AND EYEGLASSES, acts like an ordinary solar, and in this state, of course, I call it a "*Solar* ACHROMATIC MICROSCOPE," because its optical part consists of one or more achromatics combined, *acting without the aforesaid body and eyeglasses*, which would make it magnify too much to form an image on the wall of an apartment.

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* I have frequently substituted the word "*Aplanatic*" for "*Achromatic,*" as being more expressive of the nature of a combination, *in which there is no error*, either of sphericity or refrangibility; a glass may be perfectly "achromatic," *i.e.* free from colour, yet good for nothing, from spherical aberration.

The following is the version of this affair given by Sir D. Brewster in a note to his account of my "*Solar Camera Microscope*" (as he calls it) at p. 115.

"Dr Goring calls this instrument a *Solar Engiscope*, while he gives the name of *Solar Microscope* to the *same* instrument, when used in a dark room in the common way. *The introduction of the image into a camera, becomes thus the reason for changing a microscope into an engiscope.* The word *engiscope*, however appropriate it may be, as a companion to the word *telescope*, is quite inapplicable to any kind of solar microscope."

On the Expansive Action of Steam in some of the Pumping Engines on the Cornish Mines. By WILLIAM JORY HENWOOD, F. G. S., Secretary of the Royal Geological Society of Cornwall, H. M. Assay-Master of Tin in the Duchy of Cornwall.*

THE experiments which it is my purpose to describe, were instituted with a view to the determination of the quantity of steam employed, and the mode of its distribution on the working-stroke; the duty performed with a given quantity of fuel; and the work accomplished for a certain expense.

I. *The quantity of steam employed, and the mode of its distribution on the working-stroke*, were approximated to by the use of an indicator, lent me for the purpose by Robert Were Fox, Esq. It consists of a brass cylinder about eleven inches long, and 1.6 in diameter, open at both ends, and accurately fitted with a piston, which, when at rest, is retained near the middle of the cylinder by a spiral spring, of which one end is attached to the piston, and the other to the top of the cylinder: the upper extremity of the piston-rod is provided with a receptacle for a pencil. A tapered stopcock is fixed on the lower end of the cylinder, and is introduced into the grease-hole or other aperture in the cylinder-cover of any engine on which the indicator is placed. A light frame of wood, about eighteen inches long and four inches wide, is fastened to the top of the indicator-cylinder, and in it a small board slides horizontally in grooves.

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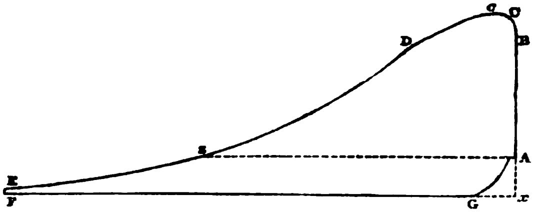
* Slightly abridged from the second volume of the Transactions of the Institution of Civil Engineers.

During the working-stroke of the engine, a direct motion is given to the slider by means of a string which passes over a pulley, and is connected with the radius-rod of the parallel motion. Its return is effected by the action of a counterpoise suspended over a similar small wheel. On this moveable board a piece of paper is firmly secured, and a pencil is placed on the top of the piston-rod of the indicator.

Let us now examine the operation of a single-acting engine, and the movements of an indicator fixed on it.



Every thing being at rest, the piston of the engine at the top of the cylinder, and the point of the pencil standing at A, steam is admitted from the boiler above the piston of the engine; the piston of the indicator is forced upwards, and the line AB is described by the pencil. The engine now begins to move, but so slowly that the steam enters from the boiler more rapidly than the piston recedes before it; its pressure in the cylinder, therefore, still increases, and the piston of the indicator continues to rise: but as the working-stroke of the engine commences, the slider moves in the direction GF, and the compound of the two motions generates the line BC. At C the space left by the descent of the piston is exactly filled by the steam, which enters from the boiler in the same time; the indicator piston, therefore, does not stir; but as the engine moves, the slider still advances in the same direction

(GF), and the horizontal line C*c* is produced. The piston now acquires speed, whilst the steam (in the boiler having expanded) enters the cylinder with diminished velocity, and is insufficient to fill the enlarging space and still retain the same density: it therefore expands, and the piston of the indicator descends, whilst the slider still moves in the same direction, and the curve *c*D is delineated. At D the steam-valve, through which the steam from the boiler enters the cylinder, is closed, but the piston of the engine still descends by virtue of the elasticity of the steam already introduced, and of the momentum acquired by the moving parts of the machine. Whilst the steam expands, the indicator-piston descends, and as the same horizontal motion of the slider still continues, the parabolic curve DE is made by the pencil.

The equilibrium valve, which connects the upper part of the cylinder with the lower, is now opened; and as the steam thus presses equally on both sides of the piston, the working-stroke terminates, and the return stroke is made: the motion of the slider is at the same time reversed.

But when this valve is opened, the pipe which connects the top of the cylinder with the bottom, and consequently a larger space, is open to the steam; and as the slider remains for the instant stationary, the indicator-piston descends through the small vertical line EF.

The return stroke is effected by the weight of the pump-rods alone; the pressure of the steam contained in the cylinder, therefore, remains unaltered, the indicator-piston is unmoved, and the line FG, described by the pencil, is perfectly horizontal.

But shortly before the termination of the return stroke, the equilibrium valve is closed, and the steam in the cylinder not being of sufficient elasticity to sustain the load of the engine, that portion of it which is contained between the upper surface of the piston and the cylinder-cover is compressed between them by the ascent of the former, until it is of force enough to support that weight; the return stroke is thus terminated, and the engine stops an instant or two before it commences another working-stroke. This compression of the steam contained in the upper part of the cylinder, forces the indicator-piston upward; and the resultant of this gradual elevation, and of the continued retrograde motion of the slider, is the small curved line GA, the pencil at the end of the stroke returning to and standing at A.

It is evident that the form of the portion ABC*c*D, which is produced during the admission of steam from the boiler on the piston, must depend on the load of the engine, its size, the dimensions of the steam-valve, the pressure of steam in the boiler, and the capacity of the boiler itself, and that it will, therefore, vary as these particulars may differ.

The only case in which I have been able to submit the results thus obtained with the indicator to a direct comparison with the quantity of water evaporated in the boilers was at Huel Towan, where 847.5 cubic feet of water were converted into steam. This would give 342,858 feet of steam of a pressure of 64.1 lb. on the square inch, (or 49.1 lb. on the inch above the atmosphere), the mean pressure in the boiler during the experiment, or 2,153,647 cubic feet of the pressure of 10.2 lb. on the inch.* The capacity of the cylinder-nozles and other parts of the engine which required to be filled with steam from the boiler at every stroke, was 355.57 cubic feet,† and the number of strokes made during the observations 7881. Therefore, if it were indispensable for the steam on the piston, at the termination of the working-stroke, to be of elasticity sufficient to sustain the load of the engine, 2,802,247 cubic feet (of a pressure of 10.2 lb. on the inch) would have been requisite; whereas but 2,153,647 cubic feet only could be obtained from the quantity of water evaporated. Consequently, but the 0.768th of the contents of the cylinder, &c., could, on an average, have been filled with steam of that force; and the remaining 0.232 of the stroke, must therefore have been performed by virtue of the momentum acquired by the machine in the early part of the working-stroke.

II. *The duty performed with a given quantity of fuel.*—The experiments with an object to determining the duty performed with a known quantity of fuel, were made on Wilson's engine at Huel Towan; on Swan's engine at Binner Downs mine; and on Hudson's engine at East Crinnis mine.* These were among the best engines in Cornwall, and they were selected on account of the very varied circumstances under which they worked.

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* 10.2 lb. was the load of the engine per square inch of the area of the piston.

† Brewster's Edinburgh Journal of Science, O. S. IX. p. 160.

At Huel Towan, the cylinder with its cover and bottom were surrounded with a case or jacket, filled with dense steam from the boiler; and these, with the steam-pipes, nozzles, &c., were covered with saw-dust from sixteen to twenty inches deep. The boilers had a layer of ashes, of about the same thickness, placed on them.

There was no steam-case at Binner Downs, but there were small fires on each side of the cylinder, and the flues from them were carried spirally round it; another little fire was placed beneath the steam-nozzle, from the boiler, and its flue was passed over the cylinder-cover; under the steam-pipe from the boiler was a similar fire, and its smoke was conveyed round the pipe for some distance. Such parts of the engine as were not enveloped by the flues were surrounded with saw-dust,† and the boilers were covered with ashes as at Huel Towan.

The engine at East Crinnis had neither steam nor heated air passed round it; but every part which contained dense steam was surrounded with a very thick covering of saw-dust, and the boilers were protected in a similar manner to those of the other engines.

On all these the indicator was placed; and also on Burn's engine at Binner Downs, which is enclosed in a similar manner to Swan's engine on the same mine, already mentioned; and on Trelawny's and Borlase's engines at Huel Vor, both which have steam-cases and other coverings like that described at Huel Towan. On the duty of these no experiments were made.

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* The engineers were respectively, Mr Grose, Messrs Gregor and Thomas, and Mr Sims.

† In the progress of my experiment, the saw-dust on the cylinder-cover ignited several times. The influence exercised on the steam within the cylinders, by the media with which they were surrounded, may be discovered by an inspection of the diagrams. (Figs. 4, &c. Pl. IV.)

TABLE I. (CONSTANTS).—*Dimensions of the Engines, and amount of their Loads.*

MINES AND ENGINES.	Diameter of Cylinder.	Stroke in		Air-Pump.		Diameter of Valves.			Total load of the Engine.	Load per square inch of area of piston.
		Cylinder.	Pump.	Diameter.	Stroke.	Steam.	Equilibrium.	Exhausting.		
	Inches.	Feet.	Feet.	Inches.	Feet.	Inches.	Inches.	Inches.	lb.	lb.
Huel Towan, Wilson's .	80	10	8	36	4	8	12	16	69666.4	10.2
Binner Downs, Swan's .	70	10	7.5	33	4	9	12	16	{ 724.3* 51967.7 }	10.23
―――――― Burn's .	64	9.33	7.75	25	4.66	7	12	13	41345	10.7
East Crinnis, Hudson's .	76	10.25	7.16	two, each 26	4.5	10	14	16	74086.1	11.4
Huel Vor, Trelawny's .	80	10	7.5	two, each 24	3.5	9	14	16	98770	14.7†
―――― Borlase's . .	80	10	8	two, each 24	4	10	14	16	76010	12.1†

* The stroke in this pump is but 5.5 feet.

† From Captain Lean's "Monthly Reports."

TABLE II. (VARIABLES).—*Quantities of Water and Steam, Pressures of Steam, and Temperatures.*

MINES AND ENGINES.	Water in Boilers.* Cubic Feet.			Steam in Boilers.* Cubic Feet.			Pressure of Steam in the Boilers, lb per square inch.			Temperature of Hot-well.†			Temp. of Condensing Water.			Temperature of Boiling-shed.			Temperature of Engine-room.			Temperature of External Air.		
	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.
Huel Towan, Wilson's	1096	984	1080	796	684	700	77.25	47.25	64.1	100°.5	90°	93°.8	66°.5	62°	64°.72	78°.5	74°.75	76°	77°.5	70°	75°.28	56°.5	52°.5	53°.84
Binner Downs, Swan's	686	586	636	400	300	350	74.78	58.07	67.87	98	84	89.24	56	50	52.32	108	73.75	76	73	64	66.48	56.5	49	52.56
―――――― Burn's	――	――	884	――	――	230	――	――	55	――	――	――	――	――	――	――	――	――	――	――	――	――	――	――
East Crinnis, Hudson's	2000	1820	1920	730	550	650	36.82	26.32	31.68	90	86.5	88.2	67.5	66	67	68	64.5	66.2	64	55	61.8	50	40.75	45
Huel Vor, Trelawny's	――	――	1164	――	――	792	――	――	47	――	――	82	――	――	――	――	――	――	――	――	――	――	――	――
―――― Borlase's .	――	――	2290	――	――	734	――	――	40	――	――	――	――	――	――	――	――	――	――	――	――	――	――	――

* The boilers were, of course, always full of water and steam; and as the quantity of one increased that of the other diminished, and *vice versa*.

† As the pressure of the steam in the boilers increased, the temperature of the hot-well declined; so that by observing the alteration in one, that of the other could be predicted with great certainty.

Note to Table II.

The following are the dimensions of the heating surfaces of the boilers of the three engines which were the principal subjects of my experiments. I add those of Loam's engine, on the United Mines (with which I have been favoured by William Francis, Esq., the scientific director of that extensive mining establishment), as the only machine the evaporation in which has been published. See Mr Lean's Report in the *Cornwall Polytechnic Society's Transactions*, iv. (1836) p. 34.

MINES AND ENGINES.	Area of the Fire-grates.	Surface exposed to action of the flame.	Total heating Surface exposed.
	Feet.	Feet.	Feet.
Huel Towan, Wilson's engine	72	114	2600
Binner Down's, Swan's ...	48	76	1440
East Crinnis, Hudson's ...	37.5	57	2500
United Mines, Loam's ...	49.5	98	2310

Loam's engine, at the United Mines, has the steam cylinder of eighty-five inches in diameter, the stroke in it is ten feet, and in the pump 7.5 feet; the load is about 12 lb. per square inch of the area of the piston, and the velocity about 4.8 strokes per minute: the elasticity of the steam employed I am unable to state. From the 2d of March to the 5th of August 1836, the duty was about sixty-five millions of pounds lifted one foot, by 100 lb. of coal, and the evaporation by the same quantity of fuel for the same period was 15.4 cubic feet. This is a sufficient approximation to the result which I had five years previously obtained at Huel Towan.

The stroke in the cylinder of Loam's engine is estimated at ten feet; an apparatus is fixed on it for registering the actual space passed over, and the mean for five months was 9.913 feet.

TABLE III.—(CONSTANTS.) *Dimensions of the Pump.*

	Huel Towan, Wilson's Engine.			Binner Downs, Swan's Engine.			East Crinnis, Hudson's Engine.		
	Length of Pump.	Diameter of Pump.	Temperature of Water in Pump.‡	Length of Pump.	Diameter of Pump.	Temperature of Water in Pump.‡	Length of Pump.	Diameter of Pump.	Temperature of Water in Pump.‡
	Feet.	Inches.	°	Feet.	Inches.	°	Feet.	In.	°
First lift, or set of pumps, from the surface ...	265.75	18.	71.125	21.25*	10.	89.24†	39.16	18	63
Second...............	263.75	15.875	71.75	242.66	18.875	72.5	159.25	18	63
Third	197.75	16.125	71.875	...	...	...	269.66	18	63
Fourth...............	113.66	16.125	72.25	...	...	...	198.583	17	62.5
The deepest, which reaches to the bottom of the shaft ...	58.16	12.5	74.	248	17.125	74.	73.25	14	63

* The stroke in this pump is but 5.5 feet.

† This *lift* took its supply from the hot-well.

‡ No correction has been applied for temperature, nor for impurities con-

The whole loads of the three engines of which it was intended to ascertain the duty, were raised perpendicularly, except the deepest lift of Wilson's engine at Huel Towan; and this was inclined to the horizon about 70°; and was connected to the engine-rod by a chain passing over two small wheels respectively of nine and sixteen inches in diameter.

The lowest lifts at Huel Towan and East Crinnis were lifting pumps, and their loads were raised by the working-strokes of their respective engines. All the other pumps were forcing pumps (plungers), and their columns were lifted during the return-strokes of the engines by the weight of the rods.*

At Huel Towan, from the surface to a depth of about 534 feet, the connecting rods were fourteen inches square; and from that place downward they extended about 300 feet, and were twelve inches square. They were kept in their places by thirteen sets of guides, which exposed a surface of about 53.5 square feet.†

From the surface to 396 feet deep in Binner Downs, the rods were fourteen inches square; and from thence downward, there were about 258 feet of twelve-inch rods; these were also retained by thirteen sets of stays, having an area of about 35.6 feet.

The rods, from the surface to 470 feet deep in East Crinnis, were fifteen inches square, and thence about 200 feet deeper they were twelve inches: eleven sets of stays retained them in their places, and exposed a surface of about 38.8 feet.

Where the rods touch the stays, they are protected by thin

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tained in the water. At Huel Towan I found, by evaporation, that about 360 grains were contained in a cubic foot. The temperature is higher as we descend; which adds to the already abundant evidence of the great heat prevailing in the interior of the earth.

* The rods are usually very much heavier than the column of water, and a counterpoise is applied to balance some part of their weight: such was the case in all the engines here mentioned.

† The lengths of the *lifts* and of the rods do not coincide, because the former overlap each other in every case, in order that the higher pumps may draw out of the same cisterns into which the lower empty; and because the rods which take the different *lifts* are also doubled at the *sets-off*.

planks of some hard wood, which are always well covered with grease; they seldom fit very accurately.

TABLE IV.—*Duration of the Experiments, number of Strokes made, Materials consumed, &c.*

MINES AND ENGINES.	DURATION OF EXPERIMENTS.	Coal consumed.		Proportion of moisture in total weight.	Quantity of oil used in the Engine.	Quantity of Grease used.		No. of strokes made by the Engine.	Strokes per minute.	Duration of the working-stroke.	Duration of the return-stroke.	Interval between strokes.	Total quantity of water evaporated.	Water evaporated by 100 lb. of Coal.
		No. of measured bush.	Total weight.			In Eng.	In Shaft.							
	1831. h. m.		lb.		pint.	lb.	lb.			s.	s.	s.		cu. ft.
Huel Towan, Wilson's	From 22 Nov. 2 32 P.M. To 23 Nov. 3 5 P.M.	50	5003	$\frac{1}{16}$	1	17	3	7881	5.35	1.6	4.8	4.8	847.5	16.95
Binner Downs, Swan's	From 8 Dec. 10 59 A.M. To 9 Dec. 0 2 P.M.	60	5561	$\frac{1}{10}$	1	12.5	3	11258	7.49	1.34	4.23	2.4		
East Crinnis, Hudson's	From 30 Nov. 9 28 A.M. To 1 Dec. 7 55 A.M.	34	3005	$\frac{1}{21}$	1	12	5	4717	3.5	1.7	4.17	11.2		

The engines were taken without any previous preparation, and they were worked without intermission, at a speed just sufficient to keep the mines clear from water; but without permitting the pumps to draw air *(go in fork)*. The workmen exercised their own discretion in the mode of working; for I purposely abstained from any other interference with them than was sufficient to satisfy myself that every thing was exposed to my notice, and fairly and honestly performed.

The results will appear in

TABLE V.

MINES AND ENGINES.	Weight of the Bushel of Coal.		Duty (in lb. lifted one foot high) performed by each bushel of Coal.*		
	As taken from the heap.	When dry.	Bushel measured.	84 lb. as taken from the heap.	84 lb. dry
	lb.	lb.			
Huel Towan, Wilson's	100	93.8	86,585,079	72,687,853	77,533,710
Binner Downs, Swan's	92.6	83.4	73,877,810	66,956,572	74,395,923
East Crinnis, Hudson's	88.3	84.1	73,954,606	70,003,555	73,502,699

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* These numbers are on the assumption that each pump delivers the full computed quantity; but in an experiment at Huel Towan, made by Sir John Rennie and myself, the *actual* compared with the calculated delivery was as 0.924 to unity. I have repeated the comparison at the same place, with a similar result.

III. *The work accomplished for a certain expense.*—The foregoing details supply all that is requisite for this inquiry, except the prices of the materials consumed; these were coal, at the rate of forty-one shillings for 72 measured bushels;* grease, forty-five shillings and sixpence per 112 lb.; and oil, four shillings and twopence per gallon; at which rates the results were by

Huel Towan, Wilson's engine,	1085 tons;
Binner Downs, Swan's engine,	1006 tons;
East Crinnis, Hudson's engine,	870 tons;

lifted one foot high for the expense of one farthing.

As supplementary to the general object of the first part of this inquiry, it may be useful to compare the maxima of pressures which obtain in the cylinders, with known elasticities in the boilers; the loads of the engines remaining unchanged.

TABLE VI.—*Load of Engines, and relative pressures of Steam in the Boilers and Cylinders.*

MINES AND ENGINES.	Lead on the Piston in lb. per square inch of its area.	Pressure of Steam in lb. per square inch.	
		In the Boiler.	In the Cylinder.†
Huel Towan's, Wilson's	10.2	61.8	27
Binner Downs, Swan's	10.23	74.78 58	26 25
.... Burn's	10.7	55	30.5
East Crinnis, Hudson's	11.4	36.8 26.3	25 21
Huel Vor, Trelawny's	14.7	47	30.5
.... Borlase's	12.1	40	30.5

Many subjects which are yet undetermined have pressed on my attention during these experiments; among which, the *steam-case* and *air-pump* are not the least important.

If any condensation take place in the case, when protected from the influence of the external air, it must be by radiation to the rarer steam within the cylinder. Now, such influence, if exerted during at least two-thirds of every stroke,‡ would not only not increase the force of the engine by adding to the elasticity of the steam, but would render requisite the injection of a larger quantity of cold water into the condenser to effect condensation, and thereby add to the burden of the air-pump.*

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* The bushel measure with a heaped *head* is the same which was used in Mr Watt's time, varying only as prescribed by law.

† All the pressures mentioned throughout this paper are absolute, and as if acting against a vacuum.

‡ See Table IV.

There must be a point at which the resistance of vapour not abstracted, to the descent of the piston, and the pressure of the atmosphere on the air-pump whilst discharging its load, are at a minimum. Beyond this, if it be attempted to reduce the force of the vapour, by injecting more cold water, the burden of the air-pump is increased by the exposure of its piston to the atmosphere for a longer time during its discharge; whilst, on the other hand, if it be sought to lessen the duration of atmospheric pressure on the air-pump, by diminishing the quantity of cold water introduced into the condenser, the increased elasticity of the unabstracted vapour offers a greater resistance to the descent of the piston.†

This subject presents many inviting topics of inquiry; but the pursuit of them, and the earlier preparation of the details‡ which I have now the honour to submit to the Institution, have been prevented by more pressing occupations.

4 CLARENCE STREET, PENZANCE,
August 30. 1837.

On Water-Spouts. By HANS CHRISTIAN ŒRSTED, Professor of Natural Philosophy in the University of Copenhagen.

ALL naturalists, except those who have themselves proposed an explanation of the water-spout,§ are agreed in thinking that science has hitherto given us but little satisfactory information on the phenomenon. This may, in some measure, long continue to be the case, if we desire a perfect explanation of the first change in our atmosphere by means of which a water-spout is caused. It appears, that, owing to the overstrained regard paid to this higher demand, which must so often be left unsatisfied, the simpler but yet fruitful labour has been neglected, of bringing together the remarkable appearances with which observations on the subject have furnished us, and by this means ascending gradually from the effect to the *proximate* cause, until at last we may perhaps succeed in ascertaining clearly the bearings of the whole matter, although, at the same time, much, in reference to the *ultimate* cause, may still remain wanting to satisfy our desire of information. It appears to me, that, by following this less ambitious course, we may advance nearly as far in our knowledge of water-spouts as we have done in respect to thunder-storms, wind, rain, and many other natural phenomena; inasmuch as we can probably specify with tolerable certainty the power by which they are produced, although we cannot accurately determine all the circumstances connected with the principles by which the action is caused at a given place with a given degree of intensity.

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* Brewster's Edinburgh Journal of Science, O. S. IX. p. 162.

† Ibid. x. p. 40.

‡ A short notice of these experiments appeared in Brewster's Edinburgh Journal of Science, N. S. VI. p. 246.

§ As we have no English term exactly equivalent to the German *Wettersaule*, we have employed the name of *water-spout* in its place, throughout the present article.—ED.

I have collected the chief features for my description of the phenomenon, from numerous scattered descriptions, for which we are indebted to observers in different ages and in different quarters of the globe; and I venture to hope that the combination of facts thus elicited will keep us free from many errors, in which most of those have been involved, who have hitherto endeavoured to explain water-spouts. It is quite possible that I may have overlooked circumstances which would tend to explain the subject, or that I may have misunderstood some of the facts contained in the descriptions; but this can easily be remedied by the obliging communications of others, whenever we possess a general analysis of the facts.

General Nature of Water-Spouts.—The water-spout is a strongly agitated mass of air, which moves over the surface of the earth, and revolves on an axis, of which one extremity is on the earth and the other in a cloud. From this cloud a continuation proceeds downwards, which forms the upper portion of the water-spout; while the lower portion, besides air, consists sometimes of water, sometimes of solid portions, according as the water-spout passes over land or over water. Some have separated water-spouts over the land and over the water from each other, but this creates confusion, for water-spouts have been observed which were formed over water and advanced over land; and *vice versa* we have accounts of water-spouts which were formed over land, and afterwards were suspended over the surface of water. They have also been seen cutting right across a river, and then continuing their course over the land; or crossing straight over an island, and then proceeding over the sea. The hitherto generally employed term *wasserhose* (water-spout) seems to me to be not altogether a correct one, and I have therefore made use of the less common one "*wettersäule*" (literally storm-pillar), although perhaps the name *wirbelsäule* (whirl-pillar) or *luftwirbel* (whirlwind, air-whirl or vortex) might be equally appropriate.

Form of the Water-Spout.—The uppermost portion is almost always wider above than below; and has sometimes the form of an inverted cone, sometimes of a funnel, and sometimes of a somewhat twisted horn. The middle portion is commonly much narrower, is frequently bent, and sometimes exhibits opposite sinuosities. The lower portion is apparently much widened, but probably only apparently so, owing to the portions of water and earth hurled round itself by the vortex. Occasionally water-spouts present expansions or contractions, but these instances are only exceptions from the general rule. Generally there is only one water-spout suspended from one cloud, and it is only now and then that there are several; on one occasion no less than fourteen were noticed, all of which seemed to belong to one and the same cloud.

Dimensions of Water-Spouts.—The height of water-spouts has been very variously estimated. I have been able to meet with no actual measurements, and have only seen accounts founded on mere calculations by the eye. A height of from 1500 to 2000 feet has been assigned to most water-spouts; but some have been seen at such distances, that the height cannot have been less than from 5000 to 6000 feet. Some observers have given a very low estimate of the height, reducing it even to

30 feet; but, in such cases, the lower part of the pillar has been undoubtedly mistaken for the whole. This might easily happen to a person who was not possessed of proper information regarding the phenomenon; for, when a water-spout begins to be formed, especially over water, there is often seen a pillar of water or of drops of water, rising from the surface› without a particular connection with a cloud being observable; but this connection is to be found, if it is sought for, and supposing we do not imagine that the cloud must necessarily be perpendicularly above the water-spout. Should such a water-spout in the act of formation be afterwards interrupted in its development, its base might easily be mistaken for the whole. It is apparent from all the circumstantial accounts we possess of water-spouts, that their upper portion is a cloud.

The diameter of water-spouts is very various. The lower portion has generally a diameter of some hundred sometimes above a thousand feet, but often much less. The vortex of drops or solid particles which the water-spout whirls along with it, has, however, been sometimes included in the mass forming the lower portion. But those cases are to be regarded as exceptions, where the diameter of water-spouts has been measured by the hollows they have formed in the earth, which afford a much less considerable size. The diameter of the middle portion is often estimated at only a few feet, but this has chiefly been by inexperienced observers. It will be made probable from what is to follow, that the middle portion of the water-spout is surrounded by a whirlwind, which does not allow of observation, owing to its containing no opaque particles.

Colour and Transparency of Water-Spouts.—The colour most frequently assigned to water-spouts is grey, dark blue, also dark brown, and fire-red; from which it would seem that the colours are the same which the clouds assume in their different states of illumination.

The middle portion of water-spouts is often transparent, but this holds good only in those which occur over water. One water-spout was noticed whose middle portion was opaque while it traversed the land, but became transparent when it proceeded over a river. The transparency of this portion at sea has sometimes been observed to so great an extent, as to allow of those clouds being seen through it which were lighted up by the sun. When an opaque water-spout begins to become feeble, the cloud-like portions, which had descended into it, retire, and as the drops of water, the foam, the dust, &c. which caused the opacity, are no longer driven upwards to so great a height, the middle portion becomes transparent.

Duration and Movements of Water-Spouts.—Water-spouts generally last longer the larger they are; but they rarely continue for half an hour, and there is hardly one example of an hour's duration.

Water-spouts seldom, if ever, remain the whole time at one place. There is great inconstancy in their rapidity and direction. They sometimes have so great a rapidity as to move seven or eight German miles (thirty-two to thirty-seven English miles) in an hour; at other times they advance so slowly, that pedestrians can easily follow them, and occasionally they remain quite stationary for a time. Their course is sometimes quite straight for a long distance, but not unfrequently it is interrupted; in some instances it is zig-zag. Their course, however, has for the most part a principal direction or bearing. It has been asserted that the direction of water-spouts is most frequently from south-west to north-east, and certainly the data hitherto collected go to confirm this opinion.

Water-spouts do not remain uniformly at the surface of the earth, but alternately rise and fall; and hence we see, that, during their progress, they have in some places, torn up trees by the roots, in others, only torn away the upper portions, and that at some points they have not touched them at all. This alternate rising and sinking often becomes very evident when a water-spout traverses a plain or the sea.

The circular rapidity of water-spouts is also very variable, for frequently the eye can hardly follow it, while at other times their motion is not so violent. Almost all observers expressly mention this circular movement, and I do not find that its existence is contradicted by any who have themselves seen the phenomenon. It is true that two American naturalists, who examined the traces left by a destructive water-spout, declared that these traces exhibited no circular movement, whereas Professor Hare mentions that there was an indication of rotatory motion on a chimney. We shall see, however, in the prosecution of our investigation, that the lower part of the water-spout has no circular movement, so long as it does not touch the ground.

There has also been noticed an ascending and a descending movement in water-spouts, the one being, of course, nearer the middle than the other. In respect to the directions observed, there prevail some apparent contradictions, but these will be explained in the sequel.

Many observers have distinctly seen windings like those of a screw; and, not unfrequently, some of these spiral windings are turned right and some of them left, one winding being nearer the middle than the other. Friedrich Rabe, who observed a water-spout in Laaland, saw straw, leaves, and other light objects, raised in spiral windings without the water-spout.

Power of Water-Spouts.—The power with which water-spouts act is often very great. They have been known to move heavy cannons, and to tear up large trees by their roots. A water-spout has been seen to transport a large tree to a distance of 600 feet. They sometimes unroof houses, nay, even overthrow the houses themselves. Beams employed in the support of roofs, have been carried to a distance of 1400 feet; and entire houses, composed of wood, have been raised up and removed to new positions. On one occasion, a water-spout was seen to roll up moist linen on a bleaching ground, and to transport it, together with a beam accidentally enveloped in it, the whole weighing upwards of 500 pounds, over a house forty feet high, and to a distance of 150 feet. Objects of little weight are carried to very great distances; thus, a water-spout has been known to transport a sewing-bag about seven English miles, and a letter upwards of twenty English miles. A fish-pond has been emptied by a water-spout, and the fish scattered round its margin. On Christiansöe, a water-spout emptied the harbour to such an extent, that the greater portion of the bottom was uncovered. But the action is not always so violent. They have occasionally passed over small vessels without doing them much harm. On land, men have been carried up by them, and yet let down again unharmed. An individual, who had the curiosity and boldness to follow a water-spout, was involved in one of its spiral windings, but escaped without injury.

It is probable that, in some of the cases, where a fall of seeds, animals, and other similar objects from the atmosphere, has been noticed, the phenomenon is to be ascribed to water-spouts.

The examples already given prove clearly that there is an elevating power in water-spouts, and it could be easy to multiply them to a great extent, if we had not, at another part of this essay, to adduce many similar ones for other reasons. I shall here notice only one other instance, which is of consequence, from the care with which it was observed. On the 19th June 1835, a great water-spout passed over New Brunswick in North America. Three days afterwards, its effects were carefully investigated by three scientific men, and more especially with reference to the direction of those displays of violent action which had been exhibited. Of course, such an investigation could only discover the direction in the immediate vicinity of the earth. The water-spout followed a course from west to east, and traversed a space of about thirty-five English miles in less than fifty minutes. It was found that those trees which were overturned in the middle of its course or near it, lay with their tops towards the east, so that the existence was thus shewn of a current of air having the same direction as that taken by the water-spout. On the other hand, those trees which had fallen further out on either side, lay, it is true, with their tops toward the east, but not directly so, being at the same time turned towards the centre of the course of the water-spout. It was also discovered, that at first, an opposite direction, viz. from east to west, must have been followed at every place, for rotten and brittle trees, which must have been first overthrown, lay under the others, and were turned to that direction whence the water-spout came. This is easily explained by the supposition that currents of air, near the earth's surface, move every where towards the centre of that place in which the water-spout is for the moment; whence it follows, that, round the anterior half of the latter, streams of air must occur in which the east is the prevalent direction, while the western direction is the predominant one in the currents round the posterior half. In some places, where it appeared that the water-spout had receded for some time, and had again descended, it was ascertained, that the overturned trees were turned with their summits to a common centre. Many circumstances also demonstrated to the observers that a rarefaction of the air in the interior of the water-spout, and one of great extent, had occurred. Not only were roofs and the upper coverings of houses removed, but even floors were broken up; a phenomenon not easily explained, unless we assume that the pressure of the air from without had become very rapidly and greatly diminished, so that the expansive force of the inclosed air must have acquired a very considerable preponderance. Many other effects of this same water-spout confirm this belief. Walls and windows were often thrown or broken outwards. In one house which had suffered much from the water-spout, a bed-cover was pressed into a crack in the wall, and remained as firmly fixed as though it had been intentionally thrust into it; a pocket handkerchief likewise was found in a crack of the opposite wall. Those objects which had been transported by the water-spout, were conveyed to the north side, and to a greater or less distance, according to their greater or less weight.

Sound and Smell of Water-Spouts.—Water-spouts are often accompanied by a violent noise, which, for the most part, has been compared to the sound of many heavily laden waggons moving over a stone pavement, or to the breaking of the waves of an agitated sea against the coast; but, by some, has been said to resemble the roar of a great waterfall. Besides these great noises, a whistling or piping sound has not unfrequently been heard.

Water-spouts often leave behind a sulphurous smell, and there are examples of a disagreeable smell remaining along the whole tract traversed by them. One individual, however, who became involved in a water-spout, perceived no odour.

Situations and Circumstances in which Water-Spouts occur. —Water-spouts do not occur with equal frequency in all situations. They are more abundant on the sea than on the land; more frequent on coasts than far out at sea, or at a distance in the interior of the dry land; and they have been more often noticed in warm regions than in cold ones. They seem to occur more especially at places where calms frequently alternate with storms.

Water-spouts take place for the most part in still weather, and during unsteady winds. In the greater number of instances, storm-clouds have been remarked in the sky before their appearance. Most frequently several occur, either at the same time, or immediately after one another; and often there is observed a new one forming where another disappeared a short time previously.

We seldom read accounts of water-spouts without finding also that electrical phenomena were noticed at the same time. Lightning is almost never awanting; thunder is likewise often connected with them, and it has been remarked that the loud noise which follows water-spouts easily prevents feeble peals of thunder from being heard. Now and then, a more widely dispersed light has been seen; so that people imagined that the corn in the fields was on fire, but afterwards to their joyful astonishment found it uninjured. It has been reported of one water-spout that fire-balls proceeded from it, of which one was accompanied by a report like that of a musket. Probably, however, in this instance, electric sparks caused a deception. Frequently, great storms follow the occurrence of water-spouts; sometimes they precede them.

Water-spouts are often accompanied by hail; also by rain in large drops, either during the period of their occurrence, or shortly afterwards. The pressure of the atmosphere has been very rarely recorded by those who have described this phenomenon. In my notes I find only one instance of the height of the quicksilver in the barometer being mentioned, and this is in the observation of a water-spout which, on the 16th of June 1775, traversed the neighbourhood of the town of Eu. The height of the barometer for three days had been 28 In. 5 L. (= 30.28 English), but fell at 7 o'clock in the morning 2½ L.

(=.22 English.) At 8 o'clock the water-spout made its appearance, and about noon the quicksilver had risen to the same height at which it stood in the morning. This result is sufficiently remarkable to make us desirous of possessing further observations of a similar nature; but, as I have already said, my notes contain no others; and on referring to the books in my possession, I have found no information on this subject.

Formation and Phenomena of Water-Spouts.—In most accounts it is stated that water-spouts are formed from above. Some observers, however, expressly say that they have seen them in the act of being formed from below. *Michaud*, who, in 1789, observed some water-spouts in the harbour of Nice, laid much stress on this commencement from the surface of the sea; it will appear, however, from what is to follow, that this only seems to be the case, and proceeds from the circumstance that the whirl of wind which forms it, so long as it is not impregnated with vapour or drops of water, is not visible.

When a water-spout begins to be formed over the sea, there is generally to be observed a circular portion of the surface of which is uneven, and has a black appearance. Soon after, the water is elevated in the form of a pillar, in which a violent internal movement is observable, the height being several fathoms. It foams, and produces drops of water above, which it scatters in great quantity on all sides, so that it distinctly exhibits an ascending and descending course, which moves in parabolic curves, like spring-water ascending in a slanting direction. The internal movement has been compared to boiling, and it has been believed that this idea was confirmed by the mass of vapour and fog which generally floats above the water-spout. *De la Nux*, however, who, for forty years, lived in the Island of Bourbon, where water-spouts are extremely common, maintains that this vapour is only apparent, and that it proceeds from the great number of drops of water spurted about. He also says, that, in order to be convinced of this, it is only necessary to see the phenomenon in a proper light. It would, however, be too bold to assert that this is always the case. It is not impossible that vapours may be formed round the agitated water, if that water possessed a lower temperature than the air, and thus cooled the moisture contained in it. That this takes place, must not be assumed, until the circumstances observed shall give sufficient support to the idea.

On land, the nature of the phenomenon does not easily allow observers to see the beginning of the formation of the lowest part of a water-spout; and accordingly, I find no data on the subject in the published descriptions. The upper part is always described as proceeding as if from a thick cloud. There is often remarked only a very slight increase of size of the cloud, which, however, is gradually extended, and presents the lengthened funnel-shaped portion. Over the sea, the upper part of the water-spout has been seen to stand far from the place which lay perpendicularly over the lower portion, until its line of union approached more nearly to the perpendicular position.

Both on land and water, there has often been seen, in the upper part of a water-spout, a thin streak of vapour which seemed to sink downwards from the cloud, and afterwards maintained itself in the fully developed water-spout. This is most easily observed at sea, when the lower portion is generally transparent. Such a streak is naturally removed from observation when the water-spout is opaque. It was remarked on one occasion, that it became visible while a land water-spout was crossing a river.

We can hardly assume that the top of the water-spout is at that point, where, to the inattentive eye, it appears to lose itself in the clouds. Shortly before the appearance of that water-spout which occurred in the neighbourhood of Eu, it was observed that the clouds separated, and that some went in an opposite direction from the rest, a fact which seemed to indicate a consequent turning round. A careful observer, *Holm*, remarked during a water-spout near Copenhagen, through the openings in the lower strata of clouds, a rotatory movement in those lying above. From the upper portion of the water-spout, there proceeded white clouds which had a whirling motion like the water-spout itself.

When the dissolution of the water-spout approaches, the middle portion, especially that part nearest the earth, becomes more and more transparent. The water-spout generally breaks up in the middle. The upper portion shrinks and disappears in the clouds. It is not probable that the latter immediately pass into a state of repose. According to what has been already mentioned regarding the observations of *Holm*, it is evident, that the clouds, after all appearances of a water-spout have ceased, nevertheless retained a rotating movement. It was at the same time remarked, that not only those clouds which formed the upper portion of the water-spout, but also the rest at some distance, possessed a circular movement.

On the actual nature of a Water-Spout.—If now, after all this, we ask, what a water-spout really is; the answer would be: it is a whirlwind (Luftwirbel). By itself, a water-spout is not more visible than air itself, but those portions which are mixed either with vapour, drops of water, or solid matter, become visible.

The source of this vortex is not to be sought in the lower regions. There is no peculiarity of the earth's crust with which the phenomenon of water-spouts seems to be connected; for, they occur in countries of the most diversified constitution, as, for example, in volcanic, as well as in non-volcanic districts. In the sea also, there seems to be no condition of the water or of the bottom, on which their occurrence is dependent. Just as little can these vortices be produced by the winds prevailing at the earth's surface, for they take place most frequently in the midst of a serene atmosphere. They must, therefore, have their origin in the upper regions.

Owing to the circular motion of water-spouts, all the parts exhibit a centrifugal action towards the circumference. This force is, as is well known, a necessary consequence of the nature of rototary motion But any one even who is not acquainted with the laws of circular motion, can form a perfect idea of this matter, by taking a transparent vessel, as for example a flask, filled with a mixture of sand and water, and by some means or other turning it round on a perpendicular axis. The heavier portions will then be observed on the outside, and the lighter in the middle. Those portions which are carried to the greatest distance from the middle, are at the same time carried upwards; this takes place because the agency which drives them outwards finds a limit at the circumference, which forces the particles that are in motion to ascend, the only direction in which they can yield to the pressure. We may be easily convinced that this action also takes place in the open air, by distributing smoke in the air, from a tobacco pipe for example, and then, at a distance of one or two feet, producing a rapid rotatory movement, when the extension of the whirlwind becomes apparent by means of the smoke.

Owing to the rotatory motion, the particles in the middle must also have a centrifugal action, and there must thus arise a great rarefaction of the air at the centre. So long as the whirlwind does not reach the earth, the air must ascend, to fill up the vacant space which has been left by the particles of air proceeding outwards. The air must therefore stream in anew from all directions, so that, when it has no particularly great progressive rapidity, those objects which are carried round by it must be directed to a common centre; but when the progressive rapidity is great, the influence of both forces on the direction must be perceptible. The rotatory movement does not affect those currents flowing inwards, inasmuch as it is taken for granted in this case, that the water-spout, although very near the earth, has not touched it; for, in the latter case, the centrifugal force would also drive outwards the particles of air near the earth. So long as the water-spout does not reach the earth's surface, an ascending current must prevail in its interior, which here constitutes the elevating power. When it strikes buildings, it may very often happen that the inward flowing currents from below become either entirely or almost altogether stopped. There thus arises a great rarefaction of the air around and over the building, so that the included air must drive the windows and walls outwards, and must at the same time force upwards roofs, and other objects which have air under them.

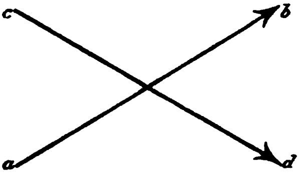
A tube of the length formed by the centrifugal force of the water-spout cannot be sufficiently filled from below by the inward flowing currents. A portion of the cloudy mass must hence descend into the vortex. It is naturally the portions nearest the middle which are driven with the greatest force downwards; nay, in a certain state of matters, the portions in sinking will be altogether stopped by the centrifugal force.

We can easily understand from all this the funnel-like shape of the upper portion of the water-spout. On the other hand, the great attenuation near the middle, may very easily give rise to the descending stripes of cloud which we so often notice in water-spouts. If the whirl of air is immediately over the sea, the water must ascend under it, partly owing to the rarefaction of the air above it, and partly owing to the air streaming in from all sides. Besides, the air contained in the water must get out, and must force itself towards the less filled space above, as always happens when the pressure of the atmosphere is diminished over water, and especially when there is rapid motion. We therefore find that the water, when the whirlwind approaches, rises up, foams, and is agitated. The greater or less proximity of the vortex must have great influence on the extent of the action.

When the whirlwind comes entirely in contact with the surface, whether it be on dry land or water, the particles of air must be sent outwards by the centrifugal force, and the currents towards the spout must consequently cease. The motion of the air is also communicated to all easily moved solid as well as liquid particles which come in contact with the vortex. They thus acquire, not only a movement outwards, but also a movement inwards. This occurs in the following manner: The circular motion extends itself downwards, and thus throws outwards towards the circumference, solid particles or water, according as the water-spout is over land or water; but, on the direct course outwards, such particles experience great resistance from the surrounding mass, so that they must ascend as they retire from the middle. This is shewn in the excavation left when the water-spout passes over loose soil, and also by the uncovering of the bottom when it traverses shallow water. It cannot be doubted, that a deepening of the sea also takes place, but this cannot be so easily observed.

On water, the combined movements upwards and outwards can be seen in great perfection, for, round the foot of the water-spout, water is thrown out in parabolic curves; nay, one observer has remarked water round the base in the form of a reversed basin. Upon the whole, it may be said that the water round the base of the water-spout forms a great wreath of elevated water, with a bubbling and foaming surface.

The particles carried up in the water-spout at the same time acquire a spiral motion, owing to the whirling which is combined with it. The falling particles, as, for example, drops or minute solid substances, which, ere the water-spout reached the earth, had been driven upwards, or descending rain-drops and hailstones, must also enter windings, which, however, cross the windings already mentioned; for movements which are ascending and descending, and which are directed to one and the same side, must cross each other as *ab* and *cd* in the accompanying figure.



Hence there are generally two spiral movements in a transparent water-spout, one to the right and another to the left.

It has been said that water-spouts over water are for the most part transparent, because they contain water; but experience proves, as well as the very nature of the thing, that in the interior there is no connected mass of water. It would be more correct to say that water-spouts which come over the sea are more rarely opaque, because they can contain no dust, and hence can only be so far opaque that they include numerous minute drops, or, what is most usual, a portion of the fog-like cloudy mass. We can, therefore, easily understand why the lower part of the middle portion of a water-spout becomes generally transparent at last, viz. because the whirling movement becomes weakened, and the cloud-funnel is hence shortened.

We have seen that the air which is immediately above a water-spout, must descend into that portion of it in which the air is attenuated, and, therefore, in the vicinity of the axis more especially. If now, as we suppose, the whirlwind extends upwards, far above the cloudy mass, in which mere observation would assign its commencement, the descending air, coming from colder regions, must condense the vapours which it meets with on its path, and partly produce large drops and partly hailstones. We can thus easily imagine that the frozen particles, during all these movements, are sometimes out of contact with warmer and moist air, and also that they are again equally often brought back to situations where they meet them, so that alternately they become so much cooled that the water by which they are coated becomes ice, or they meet moist air in which they acquire a new covering of water. Hence large hailstones may be formed, composed of various layers, the one including the other.

All this corresponds in the most remarkable manner with the facts observed. Great storms of hail and violent showers of rain almost invariably accompany water-spouts. It may, perhaps, not be too bold to suppose, that the great falls of hail, which so frequently devastate long but narrow tracts of fruitful land, are produced by great air-vortices in the higher regions of the atmosphere, or, if I may be allowed so to express myself, by water-spouts which extend beyond the lower strata of clouds. So far as I can judge, no circumstance occurs during great showers of hail, which does not harmonize with this idea. Electricity, which accompanies most hailstorms as well as water-spouts, may perhaps contribute by causing a greater variety of movements than those which arise from vortices, and thus assisting the formation of hail, so that Volta's supposition, that electricity co-operates in producing hail, here finds an application; but we should not wish to see ourselves forced to assume this co-operation, in case the presence of electricity should not shew itself so distinctly in all these formations of hail.

In the axis of water-spouts, and near it, there must also, without doubt, be a portion of watery vapour condensed. From this source, probably, is derived the rain which falls in large drops on ships that encounter water-spouts, and which has been found to consist of fresh water. The water-spout mentioned above, whose effects were so carefully noticed in North America, must also have contained water, as all objects it met with were sprinkled with mould on the west, that is the side from which it came.

When moisture is rapidly condensed, electricity is produced, and we have an opportunity of observing this sufficiently well in storms. Hence water-spouts must also be accompanied by thunder and lightning. By means of the electricity developed in water-spouts, we may, perhaps, explain the power by which, as has been occasionally observed, water-spouts alternately repel and again attract small cloudy masses. That they should be attracted by a different portion from that which repelled them, agrees precisely with the natural laws of electricity.

Although we are certain that the formation of water-spouts is accompanied by electrical action, yet we are not therefore entitled to conclude that electricity is their cause. Distinguished naturalists have expressed this opinion, but without explaining the manifold peculiarities of water-spouts. But still, even more recently, it has been attempted to explain by this cause their rotatory movement, by assuming in them the existence of a strong electrical current, which, by means of the magnetism of the earth, received its circular movement. It appears to me, however, that there is much to contradict this opinion. Although we possess the clearest proofs of the electrical nature of water-spouts, yet it seems to me not at all proved by any of the effects noticed, that they contain an actual electrical current. Individuals who have been in contact with water-spouts, never felt an electrical shock, or should a shock actually have been experienced in any instance without our being aware of it, yet there have been many cases in which it was not the case, although the human body can neither enter nor quit an electric current without receiving a shock. A decisive argument, in my opinion, which can be opposed to such a view, is, that a water-spout, whose electricity should be of such a description that the magnetism of the earth could communicate a stronger circular movement, must act very violently on the magnetic needle; now this has never been noticed in any one of the numerous vessels which have been in the vicinity of water-spouts. Even though it were to happen that *on one occasion* the needle should be affected by the approach of a water-spout, still this would by no means afford sufficient proof, for such an electric current as that assumed to exist by the theory must *always* throw the magnetic needle into considerable agitation. Hence it seems evident to me that the electricity of the water-spout as well as that of the thunder-storm, is not the cause but the effect of the natural phenomenon.

The sulphurous smell which has been perceived after a water-spout, would seem to be of the same nature as that remarked after a stroke of lightning.

The sound which so often accompanies a water-spout may be produced by the striking together of the hailstones; for this must here be very violent, and, on account of the proximity, much more easily heard than the rattling of more remote hail-clouds. The hissing noise must occur when the air is streaming into the water-spout from beneath.

The circumstance that many water-spouts are often suspended from one cloud must doubtless be explained in this way, that the cloud is not simple, but contains as many vortices as there are water-spouts exhibited. This agrees also with Holm's observations made at Stockholm in 1779, when he saw several clouds turned round in one vortex.

It is plain that the whirlwind must not necessarily remain perpendicular to the earth. Hence it follows that it may seem as if the upper part of the water-spout did not belong to the lower. Should the oblique whirlwind raise itself and approach the perpendicular line, it will appear as if the upper and under portions were approaching each other. We have examples in which the water-spout has formed oval holes in the earth. This must naturally have happened when the whirlwind deviated from the upright position.

The direction of water-spouts from south-west to north-east, may be ascribed to the circumstance of this wind preceding their occurrence.

Water-spouts are often bent; and this must arise from those winds which prevail at various heights above the earth, and which transport the entire masses of air in which the whirlwinds are contained. There is nothing in such a case to prevent one whirlwind continuing to act on the other.

It has been maintained that sharp cannon shots can drive a water-spout asunder. It is by no means inconceivable that balls which strike in such a manner that their direction is the opposite of the circular movement of those portions that they meet, can produce such an effect; but I do not venture to decide if the accounts we possess are sufficient to prove the actual occurrence of such a consequence.

In considering the water-spout, we have endeavoured to arrive at its proximate causes from the effects which have been observed and recorded; and we have ascertained that a whirlwind which begins in the higher regions of the air, and becomes expanded as it descends, constitutes the essential element of the phenomenon. It will, however, be further asked, What is the cause of this vortex? We perceive plainly that a whirlwind can be produced by two currents of air following parallel courses, but flowing in opposite directions. There is nothing to prevent us from assuming the existence of such currents in the higher regions of the atmosphere. They must often occur there while the air beneath is perfectly tranquil; at least, during an aerial voyage, a whirling cloud was met with; but we must also admit that we are in possession of no evidence of such air-currents actually existing at the period when a whirlwind is formed. But still, that this is the case seems very probable, when we reflect that they must be of frequent occurrence, and that they are capable of producing the effect we have said might be ascribed to them.

Experience teaches us, that such opposite streams in the higher regions of the air often contend with each other, while profound repose pervades the lower strata. We know also that the opposite currents produced by the inequality of temperature over the land and sea, often extend upwards to a great height, and are there in a state of great commotion, while all is tranquil beneath.

It is evident that opposite and crossing currents of wind are capable of producing also whirlwinds whose axes should run parallel with the earth. These must likewise produce great mixtures of the upper and lower strata of air, and give rise to rain and hail. This seems to correspond very well with our storms; but I am not yet able to say how far we can carry this opinion.*

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* From "*Schumacher's Jahrbuch für* 1838."

Experiments and Observations on the Temperature of Artesian Springs or Wells, in Mid-Lothian, Stirlingshire, and Clackmannanshire. By ROBERT PATERSON, M. D., M. W. S. Communicated by the Author.*

The conditions which are necessary for the production of those interesting reservoirs of water, commonly called Artesian springs or wells, are, first a more or less basin-shaped condition of the strata, and secondly an alternation in these strata of rock, some pervious and others impervious to water. Such conditions are most prominently possessed by our coal-measures, which are in general basin-shaped, and consist of alternating layers of sandstone, which is the pervious rock, and strata of coal, shale, and ironstone, which are impervious to water.

Coal being the object of most general search amongst proprietors in this country, its presence has been long ascertained by making perforations into these strata. In the course of these borings, it frequently happens that, after passing through a series of layers which are impervious to water, they come upon one from which it exudes abundantly, occasionally spouting to a considerable height from the mouth of the bore.

Thus, in an instance which Mr Johnston of Meadow Bank, near Falkirk, has been kind enough to communicate to me, upon boring to the depth of 414 feet from the bottom of one of his coal-pits, the rush of water was so great as nearly to inundate the pit, and was with great difficulty stopped for a time by inserting a wooden plug into the mouth of the bore. This, however, also proved insufficient, the plug being expelled with great force by the upward pressure of the water. The number of these springs throughout Scotland is now considerable, and, since public attention has been turned to them by M. Arago † and others, and more especially to the fact that they are the surest means of deducing the rate of increase of the internal temperature of the earth, they possess an additional interest. It is with the view of bringing together a number of interesting facts which have come under our notice regarding these springs, that the following observations have been drawn up.

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* Read before the Wernerian Natural History Society, 12th Jan. 1839.

† See Professor Jameson's Journal, vols. i. xviii. xix. xxiii. xxiv.

It has already been mentioned that M. Arago was the first who supported the idea that the temperature of the water, issuing from an artesian well, is the means least liable to give fallacious results of any which have been yet proposed, to ascertain the increased temperature of the centre of the earth.

If the water, as it makes its exit from the mouth of a bored well, possesses a temperature above that of the surrounding atmosphere, it must have acquired that temperature from the rock out of which it came. But two very important objections have been urged against its possessing precisely the same temperature as the rock which contained it. *First*, That frequently springs coming from a higher level mix, and consequently modify the temperature of the water. *Second*, That water coming from a great depth, loses a certain degree of its temperature in imparting it to the cooler sides of the perforation.

That such objections might not be brought against this species of information, we used an instrument which has been long known for ascertaining the temperature of water at great depths. It consists of a glass-tube with brass ends which screw on; in each of these ends there is placed a valve, and both of the valves open upwards. It contains a thermometer, and is surrounded with a non-conductor; a string being attached to it, it is lowered down into the bored well. The water rushing up the bore, together with the instrument descending against it, causes both valves of the instrument to open, by which a free communication through the instrument by the valves is kept up, until it reaches the bottom of the perforation. The instrument is to be allowed to remain there for a short time, and then withdrawn as quickly as possible. As soon as we begin to withdraw this instrument, the valves close, and consequently include a quantity of water from the lowest level, at which it has been.

We have used the above described instrument on many different occasions, and in many different artesian wells, both those of recent and of several years' formation, and on no occasion did we find the slightest difference of temperature, between the water taken at the mouth of the bore, and that brought up by the instrument.

In all springs, therefore, of this description, in which we can tell the depth from which the water comes, we may safely rely upon the temperature of the water at the mouth of the bore being similar to that at the bottom of it, and consequently of the stratum from which it came. And, in those instances in which we have reason to suspect that the water is mixed with springs from a higher level, the use of an instrument similar to that above described will settle the question.

At this stage of our inquiry, it may be proper to remark, that in all cases the method of ascertaining the rate of increase as we descend under the surface of the earth as deduced from artesian wells, is, by comparing the temperature of the spring with that of the mean of the district in which it is situated. This is a most important part of the inquiry, and one which requires our especial attention.

It is a very common idea and practice, to compare the heat of a spring of this description with that of an ordinary surface-spring in its immediate neighbourhood, and certainly nothing can give more fallacious results. Thus, we have seen the temperature of an artesian well, on which we were experimenting, 52°, while that of a surface-spring hard by, was at the point of freezing. It is, therefore, proper to gain as much information as we can regarding the probable mean of the place, if no thermometrical observations have been made on the district.

It is unfortunate that many of those interesting springs, which I am about to mention, occur in a district on which no ascertained mean temperature, as deduced from thermometric observations, has been drawn; and we have, therefore, been left to our own resources in fixing them, partly by their proximity to a spot, the mean temperature of which we know, partly from their level as regards, and proximity to, the sea, and partly from observations on well-shaded springs at different periods in the course of a twelvemonth.

Having thus given a few preliminary statements, for the purpose of enabling the Society the better to understand the detail of our observations, we shall now proceed to lay before them our experiments on the artesian wells of Mid-Lothian, Stirlingshire, and Clackmannanshire, with one or two elsewhere.

Our first experiment was made on an artesian spring in the immediate neighbourhood of Edinburgh. When the brewery at Meadowbank (the place to which we refer) was erected, a well was sunk to about forty feet from the surface, for the purpose of supplying the establishment with water; in consequence, however, of the supply proving deficient, the proprietors were recommended to sink a bore from the bottom of the well. This was accordingly done through different coloured clays, sands, and very thin strata of fragile sandstone, to the depth of 119 feet, till they reached a considerably thicker and firmer stratum of sandstone, from which the water filled the bore and gushed over its edges. The supply has since continued, and proves amply sufficient for the great consumption of the brewery.

The well into which the water of this bore is allowed to enter is soon filled, in consequence of the large quantity of water emitted by the artesian spring. It will, therefore, be obvious that opportunities of testing the temperature of water, at the mouth of the bore itself, do not often occur. Through the kindness of Mr Aitken, however, I was enabled to do this, and found the temperature of the water to be 49½°, and which temperature, of course, would have been the same had it come to the top of the well. This well, as we have already stated, is 40 feet deep, therefore the temperature of this water was that of the rock 159 feet below the surface.

The temperature of this spring is now to be compared with the mean of the district. It is situated at the base of Arthur's Seat, on the north side, but where no thermometric observations have been made on the mean temperature of the locality. We may, therefore, I think, very safely consider it as intermediate between that of Leith and Edinburgh, viz. 46½°, which gives us an increase of 3° of the spring over the mean of the district.

Experiments were also made in, or in the immediate neighbourhood of, the Carse of Falkirk, a very level and low lying tract of land in Stirlingshire, and in the immediately adjoining portion of Clackmannanshire. This Carse presents itself in the form of an oblong amphitheatre, which is traversed by the river Carron, and bounded on the north and east sides by the Forth.

It is composed chiefly of rocks of the coal formation, and in several parts of it, strata of coal of considerable thickness are wrought, and furnish large quantities of that mineral. In consequence of this circumstance, the proprietors throughout this tract have made extensive borings on their different estates, for the purpose of ascertaining the presence of this substance.

The first artesian spring which was examined in this locality is situated in the orchard at Kerse, the property of the Earl of Zetland. The perforation was made for the purpose of learning the thickness of the coal strata in that place. I shall annex the journal of the bore, which has been kindly put at my service by Mr Borthwick.

Section of the Strata in the bore at Kerse Orchard, 1827.

No. of Strata.		Depth. Ft.	Depth. In.
1.	**Sleek, or carse alluvium,**	41	0
2.	**Clay, with thin stones,**	107	0
3.	**Dry dark grey sandstone,**	2	3
4.	**Blaes or shale,**	1	4
5.	**White sandstone,**	8	5
6.	**Shale,**	0	6
7.	**Grey band, a peculiar kind of clay ironstone,**	0	9
8.	**Shale,**	0	8
9.	**Coal,**	0	9
10.	**Grey spreckled sandstone (micaceous),**	5	1
11.	**White sandstone,**	21	9
12.	**Grey shale,**	0	10
13.	**White sandstone,**	12	6
14.	**Shale,**	3	11
15.	**Grey blaes or shale,**	2	9
16.	**Coal,**	0	3
17.	**Shale,**	2	1
18.	**Grey sandstone,**	7	1
19.	**Shale,**	0	2
20.	**Coal,**	0	1
21.	**Grey sandstone,**	8	3
22.	**White sandstone,**	1	10
23.	**Shale,**	1	6
24.	**Coal,**	0	4
25.	**Shale,**	1	6
	Water sprung to Surface.		
26.	**Dark grey sandstone,**	9	7
	Total,	**242**	**2**

From the above table it will be observed that, after passing through a very great thickness of Carse alluvium and clay, the boring machine entered the usual rocks of the coal-formation; and that the water sprung to the surface immediately upon entering the dark grey sandstone, and rapidly increased as they descended in this stratum.

This perforation was made in the year 1827, from which time till the present the spring has continued to afford an undiminished supply of excellent water. The temperature of it, however, had not been ascertained previous to the year 1836, when it was found to be 51½°; and from that time till the month of January 1839 it has been frequently examined, and its temperature found, by the same thermometer, to be precisely similar. It may be here remarked of this spring, that it occurred to us there might be a certain loss of temperature to the spring in its passage through such an extent of alluvial matter, and this idea was further strengthened by the fact, that the water was brought from a little below the rock head, by means of iron pipes, instead of wooden ones which are usually employed. To solve such doubts, the instrument formerly described was lowered down; unfortunately it did not reach above 100 feet, in consequence of some impediment to its descent; but certainly at that depth, the water brought up had as high a temperature as that coming from the mouth of the bore itself.

Mumrills, the property of Mrs Walker, is situated on the edge of the Carse of Falkirk, and slightly elevated above it. In this estate a bore for ascertaining the presence of coal-strata was sunk in the year 1837. In the journal of this perforation, which has been obligingly communicated to me by Miss Walker, but which is too long for insertion here, the quantity of alluvial matter passed through was small, 23 feet 4 inches, and the water is brought up from the rock-head by means of a strong wooden tube. The water, it appears, never came over the edge of the bore, until they got among some thick strata of sandstone, at the termination of the bore, which is 213 feet deep. The temperature of the water of this spring has been frequently ascertained, from a short while after the borers ceased their operations to the month of January last, and on all occasions the temperature has never been found to vary, it being 51°.

It may be remarked with regard to the mean temperature of the district in which the two last-mentioned springs are to be found, and which may be considered the same with regard to those which we are just about to mention as occurring in Clackmannanshire, that, unfortunately, no thermometric observations have been made on that district.

On looking at the situation of the district on the map, we shall find that it lies about 25 miles NW. of Edinburgh, and that it is at the level of the sea, the tide often rising so high as to overflow some of the farms; and it may be remarked, that the crops are generally observed to be a few weeks later than in the neighbourhood of Edinburgh. I have tried the temperature of many of the surface springs, but in none of them, with the exception of one, did I find any thing like satisfactory results. In this spring, which is situated at a low and central part of the Carse, shaded by a few trees, and comes rippling out from among some loose stones, the temperature, in the month of April 1837, was about 45°, and in September 1838, 46½°. This difference is not great, when we consider that it is a surface spring; it might have been found to possess a much more varied temperature, had it been tried during other months; this, however, I had not an opportunity of doing. Reasoning, therefore, from the circumstances which I have just mentioned, I think we may consider the mean of this district to be about 46°.

The artesian wells of Clackmannanshire owe their origin partly, to some proprietors being anxious to ascertain if coal-strata existed on their estates, but chiefly to the circumstance of the surface-springs becoming frequently brackish from the height to which the tide rises in that locality. The first perforation which I can ascertain as having been made in the neighbourhood of Kincardine, is to be found at Loanside House; it was sunk by the proprietor for the purpose of supplying his establishment with water, free from an admixture of salt; it succeeded at the depth of 120 feet, and is now to be seen in the centre of the stable-yard of that mansion, affording an abundant supply of water of excellent quality.

The success which followed the above-mentioned experiment, induced the proprietors of a distillery which then existed at Kennetpans to sink one or two wider bores, with the view of affording an abundant and constant supply of fresh water, for the purposes of the distillery. One was consequently sunk at about the distance of half a mile from Kennetpans, and to the depth of 270 feet, before a sufficient supply of water could be procured. I was kindly informed by Mr Bernard of Kennetpans, who superintended the boring operations, that, after having passed through a compact thick stratum of white sandstone, and a stratum of shale of one or two feet in thickness, the lower part of this shale was reported to be spongy, and that the water then increased rapidly upon them. This artesian well still affords a large quantity of water, and is easily to be found by tracing up the water-course from the old seat of the distillery for about half a mile. The temperature of this water, as ascertained several times throughout the course of the last three years, has always been 51½°, which gives an increase over the mean of the district of 6½°.

In following up the same water-course, another, and, as I am informed, much deeper bore is to be found: it is immediately under Mr Bruce of Kennet's House. The particulars concerning this bore I have not been able to procure. It was made, however, it appears, much about the same time as the immediately preceding one, and to the depth of about 380 feet. It at present yields water of a temperature of 53°, which is 7° above the mean of the district.

Several other springs are also to be found, of various depths, along the same water-course, and in the immediate neighbourhood of Kennetpans. They have been sunk by proprietors to ascertain the presence of coal strata. One of them, supposed to be about 210 feet deep, yields water at 57°, others from 160 to 200 feet, 49° and 50¾°, and another, of which the depth is unknown, 51°.

I am indebted to my friend Mr Kincaid for much interesting information regarding the temperature of several other artesian wells, which occur a few miles north-west of Laurieston. They also take their origin from perforations made to ascertain the presence of coal; but as some little accident happened to the thermometer with which the experiments were made, and as the information which was procured from the borers was not of the most accurate description, I have preferred delaying the details till some future time, when more accurate experiments and information may have been procured. It may be satisfactory to observe, however, that Mr Kincaid found the temperature in all of them to be much above the mean of the district. Mr Angus of Holytown has also had the goodness to make many experiments for me on the artesian and other springs, which are to be found in the coal-pits of that neighbourhood; but as it would mix up the subject too much at present to relate them, I must also defer them till another occasion.

It has been our object in the foregoing part of this communication, to state, as concisely as is consistent with the detail of the subject, the different facts which we have been able to collect concerning the artesian wells of the districts to which we have had access; and it may not be out of place to make a few remarks which have been suggested from the consideration of the facts above mentioned.

We may remark, then, *in the first place*, that artesian wells are abundantly to be found throughout the rocks of the coal-formation. This is a fact not at all to be wondered at when we consider the variety of rocks which are to be found in that series, some of which are pervious, and others impervious, to water. Indeed, so well is this known in some parts of the country, that preparations have always to be made before commencing a perforation, so that they may be enabled to stop the flow of water from it, in consequence of the water proving a nuisance to the land in the immediate neighbourhood, if it is not wanted for economical purposes.

The quantity of the water which flows up these perforations is very various, generally bearing a proportion to the width of the bore, and the depth to which it is carried. In some of those which we have mentioned, the quantity of water is very considerable, coming out of a bore nearly three inches in diameter, in a full and continuous stream; and I am informed by Mr Johnston of Meadowbank, who continued boring to the depth of 69 fathoms (414 feet) from the bottom of one of his coal-pits, that at that depth the rush of water was so great, that they were obliged to stop the boring operations. The quantity of water ejected by this bore has been roughly calculated by Mr Johnston, to be about a hogshead per minute. The quality of the water is excellent, and is used by many families in the neighbourhood of these perforations in preference to the surface-springs. It, in general, contains a quantity of the hydrate of iron, received probably from the strata of ironstone through which it has to pass; this, however, is soon deposited by the water in the shape of a hydrated oxide, on the bottom of the water-course. In some instances, however, the quantity of iron is so great as to constitute it a slightly chalybeate spring.

It was previously mentioned, when speaking of the artesian wells of Clackmannanshire, that the height to which the tide rises causes the surface-springs to become brackish. Now, it is a curious fact connected with the artesian springs there, that some of them, and more especially those of least depth, emit a much larger quantity of water when the tide is high. This is a remark which has been made by Mr Bald, and by various individuals in their neighbourhood. It is very evident that this increase in the quantity of the water given out by the spring has a connexion with the rise of the tide, and has been most commonly, and in our opinion very properly, referred to fissures extending into the strata, and to which the sea-water has access. That no salt water can extend along these fissures is proved by the fact, that these artesian wells are never brackish; but to account for this, it has been supposed that the percolation through the strata is sufficient to remove the salt; this, however, cannot be, as no percolation which we are yet acquainted with, enables us to remove the saline ingredients of sea-water. We are, therefore, inclined rather to explain it after the following manner:—These fissures extend, as we have formerly mentioned, through the strata into the bed of the river; these, however, are constantly discharging water into the river, the quantity of which, however, must materially vary according to the superincumbent pressure. Thus, when the tide is high, the quantity emitted by the fissure into the river must be considerably less than when there is only a small quantity of water in the river. Now, if there is a constant discharge of fresh water from this fissure, we can easily understand that, according to the laws of hydrostatic pressure, the quantity emitted by the artesian spring when the tide is high, is just so much greater as the quantity of water emitted by the fissure into the river is less. This, it appears to us, will sufficiently explain the phenomenon, without the supposition that percolation, or any other process, removes the saline taste of the sea-water.

It may be remarked, in the second place, that the pervious stratum in which the water seems to be contained, is the sandstone. It is a very common idea among the borers and miners, that, in general, the water proceeds from the coal, and we can easily explain this by referring to several journals of borings, in which this is said to have taken place. The mistake of these individuals seems to be, that, in general, after piercing through a stratum of shale (which is well known to mining engineers to be a rock very impervious to water), they come to a small seam of coal, immediately under which lies the pervious stratum of sandstone. When the coal is pierced, the water from the sandstone immediately rushes up through the innumerable cracks in it, and comes to the surface. Thus, in many journals of borings which I have in my possession, and in the one which has already been referred to in a previous part of this paper, it will be seen that the water really came from the sandstone. In support of this idea I have only to mention the name of Mr Bald, who has had so much experience in mining operations, and that of Mr Johnston of Meadowbank; and the facts connected with the deep bore which I have previously mentioned as sunk by that gentleman from the bottom of one of his coal-pits, is sufficient to put this beyond doubt. "The rush of water (says Mr Johnston) occurred at the depth of forty-seven fathoms, from a stratum of white sandstone, having a seam of coal about thirty inches thick above it; the bore was continued to the depth of sixty-nine fathoms, still through this sandstone, when they were compelled to give it up owing to the rush of water."

The great difference of depth from which the water came in the different bores which I have mentioned, while the superficial distance is so small, is easily to be accounted for by the numerous shifts and dislocations which are found in coal strata, and more especially in the Clackmannanshire field; indeed, a whinstone dyke is to be seen traversing the country between two of the bores which have been mentioned.

We now come shortly to consider the increased temperature which these artesian wells are found to possess.

When we look at the experiments which have been related in a former part of this paper, we cannot but be struck with the similarity of the results, as deduced from localities far different from each other; such a result cannot be accidental, and cannot be explained by causes of a local nature, but must be produced by some general agency, which extends over the whole surface of the globe, and which is now believed by the generality of geologists to be the central heat of the earth.

It was long ago advanced by M. Arago, that artesian wells were the only sure way of arriving at any correct data by which to judge of the increase of temperature as we descend in the crust of the earth. The temperature of mines had previously been much attended to, but the results were found so various, and the sources of fallacy so numerous, that no correct data could be based upon them with any confidence. In reviewing our experiments, it will be obvious that they bear upon two very important topics connected with this subject, and these are, *first*, the rate of increase as we descend, and 2*dly*, the regular ratio which it bears.

This latter point is distinctly shewn by observing the temperature of artesian wells of small depth, and comparing them with those of great depth. Indeed, so much can this be depended on (at least in the districts we have examined), that on several occasions when individuals totally unacquainted with the subject have been told the mean of the district, and the usual rate of increase which had been found in other springs in that neighbourhood, as well as the depth of the spring we were about to examine, they have told the temperature very nearly.

The use, too, of the instrument before mentioned, renders our calculations upon the regularity of the increase very satisfactory.

We shall subjoin, in a tabular form, the result of our experiments on the temperature of the artesian wells, and the rate of increase as we descend.

List of the Localities, Temperature, and Rate of Increase of the Artesian Springs, mentioned in the present communication.

Name of Place.	Temp. of Spring.	Temp. of District.	Depth of Spring.	Rate of Increase.
	°	°	Feet.	° Feet.
Meadowbank	49½	46½	159	1 for every 53
Kerse	51½	46	231	1 ... 42
Mumrills	51	46	213	1 ... 42.7
Loanside	48	46	100	1 ... 50
Kennetpans Distillery Bore	51½	46	270	1 ... 49.1
Spring immediately under Mr Bruce of Kennet's house	53	46	350	1 ... 50
The 4 springs here noted are in the immediate neighbourhood of Kennetpans, and quite close to the roadside	51	46	210	1 ... 42
	49	46	160	1 ... 53.1
	50¾	46	200	1 ... 42.6
	51	46	210*	1 ... 42
Parish of Slamannan †	48	45	180	1 ... 60
			Average	1 for every 47.11

A simple inspection of this table will shew how very nearly the results of different localities approximate; and if we take the average number of these results, 1° for every 48 feet as we descend, we shall find that it comes very near the average, as fixed upon by the British Association, which is 1° for every 45 feet of depth. On comparing this table with the following, which has been drawn up from a variety of sources, but more especially from papers which are to be found in Professor Jameson's Journal, we shall find that the average of the former is much less than the latter, and this chiefly in consequence of some of the observations we have quoted having been made on enclosed waters at the bottom of old pits.

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* This is the supposed depth; accurate information regarding it could not be procured.

† Mr Kincaid has furnished me with this observation, on which perfect reliance can be placed.

We have altogether kept out of view the experiments of Spasky and Kupffer on the artesian wells of Vienna, as there is certainly some mistake, or rather, as Professor Bischof remarks, some militating cause, and he conjectures "that the depths of these springs are not equal to the lowest point of the course of the spring, but that the springs rise from a still greater depth."

	°		Feet.
Rüdersdorf (Magnus and Erman),	1	in every	48.3
Later observations,	1	......	51.2
Still more recent,	1	......	54.1
Erzgebirge (Bischof), from observations on enclosed waters,	1	......	61.8
Monk Wearmouth,	1	......	59.7
[*] Cornwall, from springs,	1	......	54.8
Cornwall, from enclosed waters,	1	......	52.7
Uralian Mountains, from a comparison of the most authentic observations in these regions,	1	......	55.4
Rüdersdorf, from rising springs,	1	......	54.4
Paris, Well at Port St Ouen,	1	......	72.1
Departments du Nord, et du Pas de Calais, Well of Marquette,	1	......	61.
... Aire,	1	......	41.
... St Vincent,	1	......	47.
Sheerness,	1	......	41.
Tours,	1	......	41.8
Geneva (De la Rive et Marcet),	1	......	50.
Paris, Slaughterhouse of Grenelle,	1	......	57.1
Average,	1	for every	53.1

Memoir on the Classification of Soils.[†] By M. De Gasparin.

It is undoubtedly a matter of surprise, that in an art which engages the attention of so many distinguished men,—which is the great basis of national prosperity, and which excites so many different energies, a name has not yet been devised for the expression of the different qualities of earth which form the theatre and principal material in all its operations. Attempts have been made to confer upon it a nomenclature, one of the first requirements of a rising science; but, before success can be obtained, it must make the same attainments that the other natural sciences have recently done. For the discovery of the appropriate characters of his classification, the author has directed a particular share of attention to Chemistry and Natural Philosophy, to Microscopic Observation, Geology, and Botany; and the soils have been analyzed, experimented upon, and observed in relation to all these sciences. As it is impossible, in this place, to enter at length upon the consideration of these laborious researches, we must confine ourselves to the citation of some of the facts which result from the investigation. 1st, The author points out the small quantity of carbonate of lime which is sufficient to change the character of soils. It is known that the five or six per cent. of this substance which is supplied by marling, produces very remarkable effects, whilst the one-hundredth part which is contained in the soil of Lille, as analyzed by M. Berthier, likewise sensibly affects its nature and vegetative power. Lime gradually disappears from soils, being changed into a bi-carbonate. The enclosure of *la grande Chartreuse*, which is formed of the debris of rocks which contain lime, does not now furnish a single particle of this earth. 2d, The carbonate of magnesia modifies soils in the same way as the carbonate of lime. This earth is contained in great quantities in the soils of the valley of the Nile; and those of *Bas-Languedoc* often furnish from eight to thirty-three per cent. 3d, It has often been attempted to ascertain the characters which distinguish those soils in which gypsum produces an effect upon vegetables, and those in which it has none; but hitherto without success. The author has ascertained that gypsum has no action upon recent alluvial soils, and that it produces a beneficial effect upon all more ancient soils, beginning with the *diluvian.* 4th, He has found sal-ammoniac in all the clays belonging to the vegetable stratum of soils. This observation shews the importance of clay as a magazine of the materials which favour vegetation. 5th, If by washing we separate into several portions the coarser and finer parts of earth, we find that the tenacity of such soil is in proportion to the quantity of the latter kind, except in a small number of cases. 6th, Upon examination with the microscope, it is ascertained that these exceptions are owing to a coating of ferruginous clay which adheres to the surfaces of the mineral particles,—that washing separates it with difficulty, and that it serves as a cement, forcibly agglutinating, and increasing the tenacity of the whole.

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[*] Copied and reduced from Professor Bischof's paper on Thermal Springs.

[†] Report of a communication presented to the French Academy of Sciences.—This Memoir is the first part of a work upon *Agronomy*, which is that branch of the science of agriculture which has for its object the study of soils; what belongs to their susceptibility of cultivation, and to their relative value, is reserved for another publication, the author confining himself on the present occasion to the consideration of their classification.

On the Principles of the Classification of Soils.

If we study the objects which we find in nature, that we may know them as they really are in themselves, under all the relations of their organization and their properties, it is in their innermost existence,—in the relation of their parts,—in their resemblances and dissimilarities, that we must seek for the means of grouping them together, without any regard to the circumstances which are foreign to their peculiar and proper existence. It is thus that Jussieu established the several families of plants, Cuvier those of animals, and M. Beudant his orders of minerals. Each of their groups collects together the beings or the substances which bear the closest resemblance to each other, under all the appreciable relations of their organization or of their texture, but without intermingling therewith any idea concerning their utility, which could only be considered as foreign to the subject; and this forms pure natural history.

But if we regard it in another point of view, if it be not the being or the substance in itself which we wish to study, but only such and such a property of the object, the classification then ceases to be a natural method or arrangement, and becomes a common classification. Accordingly, when we would study plants in an agricultural point of view, the consideration of families should not mislead us; as it would be impossible to establish any one agricultural principle which would be common to an entire family. That of the Gramineæ, for example, presents us with oats, wheat, rice, and the sugarcane, which require different cultures, and serve for very different purposes. Besides, the number of plants which are the objects of agricultural interest is small, and were we to conduct a course of agriculture, according to the order of families, we should have only shreds of these families, which, detached from their natural alliances, would exhibit nothing but disorder, so soon as the intermediate links were withdrawn which maintain the order of their connection. What, then, under these circumstances, is to be done? The answer is clear,—we must combine together the plants whose kind of cultivation has the greatest analogy, and we should thus have, for example, 1st, trees; 2d, the trees and shrubs which yield an annual crop (such as fruit-trees, mulberries, vines); 3d, the feculent grains (wheat, oats, buck-wheat, &c.); 4th, the plants with oily seeds (the poppy, colza); 5th, the plants which yield fodder (lucerne, spurry, ryegrass); 6th, plants used in weaving (lint, hemp); 7th, plants used in dyeing (dyer's woad or glastum); 8th, the oleraceous plants (pot-herbs, spinage, chiccory); 9th, roots (beet, carrot, madder), &c. &c. According to this method, classes are formed in which the natural affinities of plants are often broken, but which, on the other hand, present another kind of affinities, such as proceed from their mode of culture. They are, therefore, natural classes in relation to farming, whilst they cease to be so considered in the light of natural history. This is a method which has been followed in regard to medical substances, articles of food, &c. Chemistry itself classes natural bodies in a manner different from what mineralogy does, because the view it takes of them is different. Thus, not only the practical arts, but the pure sciences themselves, modify classification, according to the object they have in view, without at all changing the natural relations of bodies; they determine that one of their properties which ought to predominate in the order they impose.

In agronomy, therefore, it is no longer simple substances, or bodies in their individual condition, such as a plant, or crystal, we have to examine; but it is mixtures of many of these substances, of which we form individuals only by abstraction, as we do in rocks, formed as they are by the union of many minerals. But this intellectual operation which regards the habitual union of several substances and forms from them one collective being, is much more natural in practice, than that which would consist in considering in granite only the three minerals which compose it, without regarding their aggregation; and still more than that which, decomposing these minerals into their last chemical elements, would remove granite from mineralogy, and view it only as a compound of oxygen, silicium, aluminium, potassium, magnesium, and iron. It is thus too with soils, although some may present only a single mineral element, as for example, silex; and though others, as much oftener happens, contain many, and these associated with vegetable and animal debris. We can consider abstractedly each of these mixtures as a pulverized rock, and deal with it, as we do with rocks, in forming a systematic whole.

After having thus demonstrated that both reason and custom authorize us to propose a classification of soils with a special relation to agriculture, we may examine; 1st, what the characters are which agriculture should examine in soils; 2d, the relative value of each of these characters; and, 3d, their application to classification.

§ I. *The Characters of Soils in relation to Agriculture.*

When an agriculturist devotes himself to the investigation of a soil, it is a matter of indifference to him whether it is composed of alumina and silica, or whether these substances are in the state of quartz or felspar, or that by their aggregation they form the debris of granite, or finally, that they belong to primitive, transition, or alluvial formations: what he requires, is to know what kind of plants the soil will produce with the greatest advantage, the trouble it will require to put it in a state of culture, the manuring it will need, the quantity of this manure it will yield to the plant, and the portion it will retain in its own substance; these are its agricultural characters,—those which adapt it to the objects of agronomy, and which shed light on his researches.

What we have already said of the composition and properties of soils, demonstrates that certain of their scientific elements have a relation to the properties which are inquired after by agriculture. Thus, as to the nature of the crops which may be expected from different soils, those which contain carbonates of lime and magnesia are eminently qualified to produce wheats and leguminous crops; the siliceous-clay lands are the soils peculiarly adapted to forests; the siliceous are proper for plants which vegetate in winter, as rye, &c.; mould favours the vegetation of those pot-herbs which are cultivated for the stems, leaves, &c. As regards the facility or difficulty of working soils, those that are siliceous are easily dressed, as well as those which have an organic origin; whilst calcareous and clayey present great differences in this respect, according to the diversity of their composition. Finally, sandy and calcareous soils require frequent manuring, and this addition they decompose to the immediate profit of the plants, whilst clayey ones retain the manure, may have the process of manuring postponed to greater intervals, and receiving at the same time a larger quantity of manure. Diluvian soils admit of improvement with gypsum, and siliceous clays with marl; whilst lands rich in organic matters require the dung of animals to facilitate and promote the decomposition of the mould.

Thus, we find in the mineralogical characters we have examined in detail, whether physical or geological, certain relations with the agricultural characters. There are whole groups of soils whose natural characters answer to those agricultural characters, and reproduce properties which are common to them all. After having recognised and distinguished them, we must next recognise those of them which from their importance and generality will most naturally form the primary groups.

§ II. *Relative Value of Characters.*

For the appreciating of the relative value of the agricultural characters which we have enumerated, it is necessary to discover which amongst them is the most indispensable, and those whose absence would be most hurtful to agriculture; the degree of their importance and necessity will then indicate the relative subdivisions.

The appropriation of lands to the different kinds of cultivation seems to possess these qualities in the highest degree; and here, in fact, must begin every kind of agricultural improvement. It is only after having destined a particular soil to an appropriate culture, that we can begin to consider the labour and improvement it requires. These labours and improvements will be without an object and a bearing, if we are still ignorant of the plant to which they would be useful. And, moreover, this investigation of the appropriation of soils to particular cultures, is connected with the most natural classification, in a mineralogical point of view ; it breaks the smallest number of affinities, and consequently renders the determination of soils more easy and more satisfactory.

The labour required for bringing the soil into good working condition, is also a matter of great importance ; for if the appropriation of soils decides the phytological or botanical part of the question as to cultivation, this other consideration bears on the question of economy. It modifies the plan of regulating the soils which might be determined on from the first consideration taken by itself: it has also a very decided influence upon the choice of the means to be employed in overcoming the resistance upon the number and kind of animals, and upon the implements to be procured. But were this circumstance taken as the primary basis of the classification, we should then break all the natural affinities of soils ; for all the mineralogical kinds are, in a greater or less degree, susceptible of tenacity. Besides, it is evident that this greater or less degree of facility in working soils, dissociated from their capability of producing the most useful plants, is a quality of very little value ; that it is of no great moment, for example, that we can easily labour a dry sand, and a rich marl only with difficulty ; and that, in short, in the examination of an estate, it is the character of the plants we inquire about, before we calculate the expense of their production.

As to improvements, and the necessary means of enriching vegetation, they are without doubt the sign and consummation of good farming ; but their use is much less frequent than it ought to be : most lands are cultivated without their aid ; and we cannot, therefore, consider an exception which, we trust, will soon cease to be one, in the light of a character so general as the preceding.

Upon the whole, therefore, we establish the subordination of the agricultural characters which ought to be considered in soils, in the following method :—1*st*, The appropriation of soil to plants ; 2*d*, the tenacity of the ground ; and 3*d*, the aptitude of the soil to receive certain kinds of manures or improvements.

§ III. *Primary Classification of Soils after their appropriation to particular cultures.*

The Cerealia are every where throughout Europe the basis of rural operations. They succeed more or less in all the soils which supply them with a firm support, and which at the same time allow the air to penetrate to their roots ; ranging from sandy soils which do not contain less than $\frac{80}{100}$ of sandy or rocky materials, to stiff clays, provided that the soils do not contain $\frac{8}{100}$ of sea-salt, or any sulphate of iron. The soils or pure mould are also excluded from this kind of cultivation, from the defective cohesion of their elements, and their frequent change of volume. Allowing for these exceptions, there are three principal groups of soils. 1*st*, The saliferous ; 2*d*, the sandy, which contain even as much as $\frac{8}{10}$ of sand or of rock ; and, 3*d*, organic soils, which contain $\frac{1}{4}$ of mould. This division, it should be remarked, agrees not only with the results of the mineralogical study of the soil, but also with its tenacity ; and hence it is perfectly natural.

There remains, moreover, a great number of soils in which wheat thrives, when they contain, besides, a sufficient quantity of organic matter ; but they are not all equally favourable. For its successful cultivation, those which contain only silex and clay must have lime added, a principle without which they will scarcely yield any return. As soon as this is supplied, their product immediately increases in a remarkable manner, to the extent of a fourth, a third, and even a half. The vegetation of the Cerealia, therefore, indicates a grouping which subdivides lands into soils containing carbonates (of lime or of magnesia, the latter supplying the place of the former), and into silico-argillaceous, or clayey soils, which do not contain carbonate of lime or of magnesia. Hence the agricultural principle is, in its turn, in agreement with the principle drawn from improvement, and no longer with that of tenacity; for, in these two classes of lands, we find that, according to the proportion of the mineral principles which they contain, the soils possess a different degree of tenacity.

Other kinds of culture confirm this view. Fruit-trees thrive admirably in siliceous and clayey lands, and these are generally the soils of forests; leguminous plants prefer soils in which there is a carbonate, and then they appear naturally; and it is only in the same circumstances that dye-plants afford vivid colours.

We have already remarked, that we cannot avail ourselves of the characters which are drawn from the tenacity of the soil, without destroying the groups which we had previously formed. We can, however, reserve them for the formation of secondary groups, which will subdivide the primary classes. It will be the same thing with those which are derived from the property which gypsum possesses of rendering certain soils eminently productive of vegetables; but we have seen that it is the geological position of soils, more than their composition, which has hitherto contributed to designate them. But each of our groups contains earths of different geological formations, so that we should run the risk of breaking them up if we were to introduce this consideration into the formation of our primary groups; and, upon the principle above insisted upon, this character will rank after the one derived from tenacity.

These principles being allowed, we now proceed to the exposition of the classification of soils, which they supply.

FIRST DIVISION.—SOILS HAVING A MINERAL BASIS.

Character.—These soils do not lose a fourth of their weight upon heating them till they emit no more vapour.

FIRST CLASS.—SALIFEROUS SOILS.

Character.—Soils with a salt or styptic taste, containing at least 0.005 parts of hydrochlorate of soda, or sulphate of iron.

1st, Saline Soils.—Water digested with these soils, gives a precipitate with nitrate of silver.

2d, Vitriolic Soils.—Hydrocyanate of potass gives a white precipitate with the ferruginous salt which is contained in the water digested with this soil.

SECOND CLASS.—SILICEOUS SOILS.

Characters.—Producing no effervescence with acids; affording by levigation at least 0.70 for their *premier lot.*[*]

THIRD CLASS.—CLAYS.

Characters.—Not yielding effervescence with acids, and affording by levigation less than 0.70 of the first portion.

FOURTH CLASS.—CALCAREOUS AND MAGNESIAN SOILS.

Characters.—Producing effervescence with acids; lime, or magnesia, or both, are found in the solution.

First Sub-Order.—Chalks.

Characters.—Leaving no residue after the action of the acid, or only leaving a siliceous residue less than 0.50.

Second Sub-Order.—Sands.

Character.—This soil contains at least 0.50 part of siliceous or calcareous sand, which does not escape from a sieve whose division or holes are the 0.02 of an English inch in diameter.

Third Sub-Order.—Clays.

Character.—This soil leaves a residue of 0.50 of clay after effervescence and levigation.

Fourth Sub-Order.—Marls.

Character.—After the action of acid, a clay remains whose levigation does not remove more than 0.10 of free silex.

First Section.—Calcareous Marls.

Character.—Having at least 0.50 of carbonate of lime or magnesia in their composition.

Second Section.—Argillaceous Marls.

Character.—Having at least 0.50 of clay.

Fifth Sub-Order.—Loams.

Character.—After the action of acid, the residuum presents clay and free silica, which, by their levigation, give each more than 0.10 of the weight of the soil.

SECOND DIVISION.—SOILS WITH AN ORGANIC BASE.

Character.—Losing at least a fiftieth of their weight when they are heated till they do not emit more vapour.

FIRST CLASS.—FRESH MOULD.

Character.—The water in which this mould is digested or boiled does not redden litmus paper.

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[*] The "*premier lot*" comprehends the large particles which are deposited when the water in which the earth is dissolved, is strongly shaken.

SECOND CLASS.—ACID MOULD.

Character.—The water in which this mould is digested or boiled reddens litmus paper.

In each of these classes, the *genera* are formed by the consideration of the tenacity of the soil, which is so very important an element in its characters.

The work concludes by laying down rules for the description of *species*, and with *examples* of all the methods of description. In reading these, we at once perceive how precise an idea of soils is conveyed in a manner that cannot be misunderstood by any agriculturist. The possibility of transmitting these clear and pointed descriptions to a distance, follows as a matter of course; and we shall in this manner be freed from all that vagueness which has been so long a just cause of complaint.

"If I have succeeded (concludes the author) in what I have proposed in writing this book, the study of agricultural treatises will be greatly facilitated; the different methods which are followed in distant countries will no longer appear so marvellous, and will become more intelligible; we shall comprehend better the considerations which limit or extend the several cultures, and a necessary link being established between the science of agriculture and other natural sciences, it will become more intelligible to all, and will more readily profit by the progress of all the other branches of human knowledge."

On the Geographical Distribution of Insects.

The geography of animals in general, is a department of their history which owes the degree of progress it has made, almost entirely to the exertions of modern naturalists. Their predecessors, with very few exceptions, regarded a description of the *intrinsic* properties of natural objects as sufficient to gratify all reasonable curiosity; and the relations which they bore to climate, temperature, and other physical attributes, scarcely ever attracted their regard. The subject is now, however, viewed in a more philosophical aspect, and a zoologist may have an intimate acquaintance both with the external characters and internal organization of a species, and still have occasion to lament that he is in ignorance of some of the most interesting considerations that attach to its history.

It will readily be conceived, that the distribution of the lower animals would be still less attended to. Creatures which seemed to owe their birth to some fortuitous influences, or to effloresce, as it were, from the very surface of the soil, could scarcely be supposed to be subject to the same laws which affected the condition of superior natures. Even down to the time of Linnæus and Fabricius, an era from which we are wont to date the rise of all true zoological knowledge, but little had been done in furtherance of this department. It may even be questioned, in regard to insects especially, whether the labours of the distinguished individuals just named did not tend, in some respects, to embroil rather than illustrate the subject of their geographical distribution. So habitually disregardful were they of determining the precise local habitations of the species, that we are often left in doubt even as to which of the hemispheres they belong. The phrase "ab Indis," which occurs so frequently in their works, may mean either the East or West Indies, and is occasionally used with such latitude of meaning as to be synonymous with exotic, or extra-European. This want of precision, however, it must be admitted, was in some measure unavoidable, as the consignments of insects which they received were generally collected indiscriminately from foreign countries, and blended together in inextricable confusion. Fabricius, at least, appreciated the value of accuracy on this point, and lays down some general principles, in his *Philosophia Entomologica*, regarding insect geography, which may be considered as the first formal attempt to place the subject on a sound footing. By the time of Latreille, a great accumulation of localities had taken place, the accuracy of which could be implicitly relied on, and he was enabled to deduce from the consideration of them certain general laws, which appeared to him to regulate the distribution of insect life. Of his treatise on this subject it is unnecessary to give any account in this place, as a translation of it has appeared in the Edinburgh Philosophical Journal.* The same subject was afterwards taken up by Macleay, in his *Horæ Entomologicæ*, and subsequently by Kirby and Spence, in their well-known Introduction. Since that time, the study of Entomology has been prosecuted with so much zeal, that comparatively ample materials have been placed at the disposal of those inclined to labour in this interesting field. Of these materials, perhaps, the most valuable is the well-digested catalogue of Count de Jean, which determines, with as much accuracy as the subject is susceptible of, the localities of no fewer than 22,399 species.† These, it is true, are Coleoptera alone, but aids of a similar description are not wanting in most of the other orders. M. Lacordaire, Professor of Zoology in the University of Liege, is the last writer that has availed himself of the numerous advantages which recent discoveries afford for the illustration of this subject, to which he has been enabled to add the result of his own personal observations, during a lengthened residence in many parts of the new world, where his attention was particularly directed to the inquiry. His essay we consider of so much value, in several respects, that we now propose to lay the substance of it before our readers. While the direct object of it is to determine the influences which seem to regulate the geography of insects, many of the principles advanced will, we think, tend to elucidate the distribution of animal life in general.‡

The geographical distribution of insects, like that of vegetables, may be considered under two points of view—1st, in regard to the physical nature of the localities in which they are found. Thus, some live in water or on land; some on leaves, others in carcasses—this constitutes their *station*. 2d, in relation to their geographical position, that is the country to which they are indigenous; and this is what is called their *habitation*, or, by abbreviation, their *habitat*. Under these two points of view, genera, tribes, families, and any other kind of group, can be equally well considered as individual species.

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* See Vol. V. p. 370.

† This is the number included in the last edition of the Catalogue. Since its publication, the collection has been increased to upwards of 23,000 species. It is, therefore, by far the richest private collection of Coleoptera in the world. The species of the same order in the Berlin University Museum, the most extensive that exists, amounts to 28,000 species.

‡ This essay is contained in an Introduction to Entomology, published last year in Paris, forming a portion of the work entitled *Suites à Buffon*. The first volume may almost be regarded as a translation of Kirby and Spence. The second contains an accurate abstract of the discoveries recently made in insect anatomy, a department in which Kirby's work may be considered as already, in some degree, obsolete. The most original part of the whole is the geographical essay in question.

In regard to both of them, the entomologist has the two following questions to propose to himself, and it is not till he is in a condition to reply to them that the science can be considered as complete.

1st, A locality or a country being given, what are the species of insects which are found there?

2d, A species or a group being given, what is the country or locality which it inhabits?

Previously, however, to entering upon the examination of stations and habitats, an important problem presents itself, namely, why species inhabit such and such a locality, rather than others? The only means by which it can be solved is to ascertain how far external physical circumstances act upon species; and, if these are insufficient to explain the matter, we must revert to a higher cause—that, namely, which ordained the order of things as they now actually exist.

Influence of External Circumstances on Insects.—It is not in our power to explain why the organization of living beings is, in certain cases, acted upon by physical agents, and in other cases exempted from this influence; but the fact of such a difference existing is proved by observation, and it is the fact alone which is of importance in the present inquiry.

External circumstances do not act on animals and vegetables, either in the same degree or in the same order. Vegetables are fixed to the soil, and, deriving their nourishment immediately from it and from the atmosphere, are placed in most intimate relation with the earth, air, light, water, &c. while animals having the power of motion, can, at pleasure, withdraw themselves to a certain point from the action of these agents. Insects being for the most part very active, are, of course, in the latter condition, and we must therefore take into account their faculty of locomotion.

The external conditions whose influence requires to be examined are, nourishment, temperature, light, soil, and organized beings.

Influence of Nourishment.—When vegetable life ceases, animal life likewise becomes extinct; but this is the case only with land species. Such as are aquatic are independent of vegetable life, and perhaps no part of the sea furnishes a greater number of living beings than the polar regions.* Insects are everywhere subject to the law in question, and they cease to occur in the same latitude as phanerogamous plants, with which their existence is very closely connected. Melville Island (75° N. Lat.), which possesses only a few vegetables of this sort, furnished only six insects to Sir E. Parry's expedition, during the eleven months of their sojourn upon it.

When phytophagous species cease, it follows as a necessary consequence that creophagous species, which live at their expense, must likewise cease.

The inverse of this is observed in proportion as we recede from the poles. Phytophagous species augment along with vegetables, and their number attains its maximum under the Tropics at the same time as the latter. But this progressive increase does not take place with all the creophagous species, particularly those of the coleopterous order, as will afterwards be shewn more particularly. Of these the equatorial regions possess an infinitely smaller number, viewed both absolutely and relatively, than the temperate regions of the northern hemisphere.

Other considerations may be deduced from the relations which exist between plants and insects.

It is doubtful whether any insect is to be found whose existence is so inseparably connected with that of a particular species of plant, that if the latter become extinct the former must necessarily perish along with it. Neither has it been shewn that any of them follow a vegetable species throughout the whole extent of its habitat, the one existing wherever the other exists; but it is certain that many natural groups of insects are connected with corresponding botanical groups in respect to nourishment. It is thus that the genus *Papilio*, very numerous in species, and subdivisible into a great number of secondary groups, contains, among the latter, some which live only on the citrus, others on the umbelliferæ, laurel, sassafras, &c.—whence result many consequences, some of them of direct interest in a botanical point of view. Thus,

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* The animalcules of the order Acalephus are to other marine animals what vegetables are upon land to terrestrial animals. It is their innumerable multitudes which renders life possible in the seas of these dismal regions. Mr Scoresby has calculated that a surface of two square miles contains 23,888,000,000,000,000 of these animalcula, and they are met with in greater part of the Polar Seas.

1. A plant happening to disappear from a locality, the species of insect which it nourished may have recourse to an allied plant of the same family, and thus maintain itself in the locality in question.

2. When a plant in one country supports a certain species of insect, if we happen to discover, in a very distant country, another plant of the same group, we may often conclude, *a priori*, that this country likewise possesses an insect of the same genus as the other. The numerous species of the Lepidopterous genus *Lybithea*, for example, live on the Celtis in the caterpillar state, and there is one species in the central districts of France which feeds on the leaves of *Celtis australis*. The genus Celtis likewise occurs in the Antilles, Madagascar, and Java, and in each of these countries a particular species of Lybithea has been found. It will be observed, that in such cases the existence of the plant may be inferred from the presence of the insect, as well as that of the insect from the plant, but the former conclusion is by no means attended with the same certainty as the latter.

3. When a plant is transported into a foreign country, when it finds no congeners among the indigenous vegetables, the insects of the country to which it has been carried do not attack it. Thus, our cabbages, carrots, vine, &c. acclimated in Cayenne, suffer no injury from the insects of that country; and in like manner the Indian chestnut, the tulip-tree, and the magnolia, are respected by ours, as well as the greater part

of exotic plants, which we cultivate either in green-houses or in the open air.

4. When, on the contrary, a plant has congeners into the country to which it is transported, it is subject to the attacks of the insects belonging to that country. This is the case with all the oaks, willows, and poplars of North America naturalized in Europe, and also with the pine tribe, most of which suffer when introduced from the attacks of hylurgi, &c., which are often so fatal to our indigenous fir-tree.

But although we are unacquainted with any insect which accompanies a particular vegetable wherever it may be conveyed, it often happens that, when a plant extends beyond its natural boundary, it is accompanied by one or more of the insects unsupported in its native locality. Thus, it has happened, that since pine plantations were multiplied in the vicinity of Paris, the *Lamia ædilis*, (the timmerman of the Swedes and Laplanders, which they regard with a kind of religious veneration), an insect of Northern Europe, and previously unknown in that part of France, has begun to make its appearance. The corn weevil (*Calandra granaria*), in like manner, seems to exist in whatsoever place the cereal grasses have been transported to.

Influence of Temperature.—Temperature acts on insects mediately and immediately; in the first instance, mediately by its influence on vegetation, which is destroyed by an excessive cold, and fostered by heat. The creo-saprophagous species, that is, such as live on decomposed animal substances, are not exempted from this influence, for the rarity of the species in intertropical regions, formerly alluded to, probably arises from the rapid decomposition of carcasses occasioned by the excessive heat, which makes them entirely disappear almost in a few hours. Whence, it follows, that only such species as are developed with extreme rapidity, the muscidæ, for example, can alone live in these climates. The coleoptera, whose growth is much more tardy, would have no time to operate upon a dead body, and it is in this order in particular, that the rarity of which we speak is most notable.

The direct effects of temperature are not less important, although they do not act so powerfully on insects as on plants. The latter, in fact, require a determinate degree of heat for a certain period, and can resist only a certain degree of cold. In absence of the former, the seeds fail to ripen ; if the latter be exceeded, the plant is destroyed. In these respects, insects are not confined within such narrow limits.

It is true that they also require a certain temperature at a particular period of their lives, that, namely, of their transformation into perfect insects ; but this period may be prolonged almost to an indefinite extent, without compromising the life of the animal. It has been proved that, by placing a chrysalis in an icehouse, its exclusion may be retarded for one or two years, and that during this stage of its life, as well as while a larva, many insects may be frozen without causing death. Now, let us suppose, that, by some sudden change in the constitution of our planet, the temperature of winter were to continue for an entire year. On the return of warmth, almost the whole vegetable kingdom would be destroyed in our climates, while insects would preserve the greater part of their species. What maintains their races, is, then, the established order according to which nearly all, as in our countries, pass the winter either in the egg, larva, or nymph state. Individuals which sometimes pass this season in the perfect condition, invariably take refuge in suitable retreats, and are thus preserved from injury, like plants protected by a covering of snow.

The extremes of heat and cold, as has been remarked by Mr Macleay, are much more essential in determining a locality than the mean annual temperature of the year. In this, we can discern the reason of certain remarkable phenomena, such as the following ; that the intertropical forms of insects are prolonged much farther north in the New than in the Old Continent, which is the reverse of what botanists have observed in regard to plants. Thus, we find in the vicinity of New York, 40° 46′ Lat. N. *Phanæus carnifex*, *Rutela sexpunctata*, *Gymnotis nitida*, and a considerable number of other species belonging to genera, essentially equatorial ; while the insects of Oporto or of Rome, situate under the same parallel, have a *facies* infinitely less, resembling that of the species of Asia or of equatorial Africa.*

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* This difference, however, it should be remarked, may partly arise from the countries being continuous, or nearly so, in one case, while African

The comparison of the maxima and minima of temperature between Rome and New York, affords the following explanation of this difference.

Mean temperature of the warmest month,	Mean temperature of the coldest month,
New York, + 28 1 R.	——— 3.7
Rome, + 25.0.	+ 5.7.

Thus, the difference between the maximum and minimum of temperature is 31°.8 at New York, while at Rome, it is only 19°.3. An insect in the former of these countries is, therefore, subjected to much greater alternations of heat and cold than one placed in the other. But, on the one hand, it passes the cold season in the state of larva or pupa, without sustaining harm from it; and, on the other, it enjoys 3°.1 more heat during the summer. If it belongs to an equatorial genus, therefore, it is placed, at New York, under conditions much more nearly allied to those of its intertropical congeners, than an insect at Rome could be.

Mr Macleay explains, on analogous principles, why the coleoptera, hemiptera, hymenoptera, &c., are so limited in species in the polar regions, while the culicidæ are generated in millions during the summer, and are even more annoying in these northern latitudes than under the tropics. In these regions winter continues nearly nine months, and the thermometer often descends to — 40° R. while it ascends during the summer to + 30° and even to 33°. But this short duration of the heat harmonizes with the short lived existence of the culicidæ, which, moreover, pass their early states in the water, where they are sheltered from the extreme cold, while the coleoptera, living for a longer period in the perfect state, require a longer continued heat, and as they pass their first stages in the earth or the interior of vegetables, they are not so well sheltered from the cold.

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forms may be prevented extending northward into Europe by the intervention of the Mediterranean. M. Lacordaire states, that equatorial forms are represented in Europe by scarcely more than two species. *Danais chrysippus*, which is found in Calabria, and *Charaxes jasius*, which extends its habitat, as far as the middle of France. *Pimelia*, *Akis*, *Scaurus*, *Brachycerus*, &c. which are found around the basin of the Mediterranean, do not belong to the forms in question, but to those of the temperate zone in the vicinity of the tropics.

Temperature at once influences habitats and stations, but its effect on the former is much greater than on the latter, for it varies more from one country to another, than in the different localities of the same country. Yet it is of importance for the entomologist to know in what the various stations differ in this respect, in order that he may regulate his researches accordingly. Thus, in our temperate climates, certain carabidæ, such as the *cychri*, frequent, in preference, places exposed to the north, while the opposite of this takes place with the *ateuchi.*

Influence of Light.—Light has little influence except on the coloration of animals, and it would have been unnecessary to mention it here, had it not been for the purpose of refuting an often repeated assertion, that insects are more brilliant in colour the nearer their habitat approaches the equator. This is true in this sense, that more brilliant species are found in intertropical countries than elsewhere, and that individuals of the same species are more brightly coloured the more southern the country they inhabit. But there is another law which seems to have regulated the coloration of these animals, viz. that species are more brilliant in those regions which are to be regarded strictly as their native country, than in any other situation. We may find a proof of this in the carabi, which predominate in the northern temperate zone; the Siberian species, which are very numerous, being fully equal in colour to those of Southern Europe and the coasts of Barbary.

According to what we observe in regard to plants, we might expect that the kinds which live on mountains would be more deeply tinted than such of their species as inhabit plains, but the contrary is most frequently the case. In the Alps of Dauphiné, for example, there is a variety of *Carabus auratus*, which is quite dull compared with the type of the species found in other parts of France.

Light has an influence on stations merely. Many species delight in almost complete obscurity; others prefer the deep twilight of forests. In general, it is those only possessing powers of easy and rapid flight, which expose themselves for a long time to the ardent heat of the sun; these are also observed to be, for the most part, more warmly coloured than any other kinds.

Influence of Soil.—As insects do not derive their nourishment immediately from the soil, the latter, considered mineralogically, can act on them only indirectly through the medium of the plants which grow in it. If there be insects, not of a fossorial kind, which are found only in calcareous soils (according to Latreille, *Licinus*, *Rhodocera*, *Cleopatra*, and many *Dasytes* are so circumstanced), it is because the plants on which they feed grow only in such earths. Consequently, an acquaintance with the vegetation of a locality, enables us, in a considerable degree, to determine what kind of insects inhabit it, even although we be ignorant of the kind of nourishment they require.

It is at the same time true, that it is possible, merely from an inspection of the soil of certain countries, to indicate *a priori* what families of insects are likely to prevail there. Thus, an arid, rocky, and above all, a saline soil, such as is found in Central Asia, Peru, and Tucuman, announces, anteriorly to all examination, the presence of Melasomas. But it is proper to remark, that in this case, in our supposed ignorance of the vegetation, it is the more imperatively necessary to take note of the temperature, for, in northern countries, tracts are to be found analogous to those of which we now speak, which do not produce a single melasoma.

With regard to fossorial species which excavate the soil in order to obtain a place of retreat, and for the purpose of depositing their eggs, it is obvious that they will frequent only such as does not offer a great degree of resistance. Each of them, however, shews marked preferences; *Sphex*, for example, burrows only in fine and very light sand; *Cicindela hybrida* prefers gravel mingled with a little vegetable mould; *Ammobatus* the beaten earth of pathways, &c.

Under these two points of view, soil has influence on stations rather than habitats, but the inverse takes place with respect to its physical constitution, that is to say, its elevation above the level of the sea, its inequalites, the water on its surface, &c.

As mountains modify the isothermal lines in the same manner as latitude, they produce the same effects on insects as the latter. It often happens that a species which frequents plains in northern regions, only recurs among mountains in countries farther south, without existing at all in the intermediate districts. Thus the *Parnassus Apollo*, whose special native country is Sweden, when it frequents plains and hills of inconsiderable elevation, is again found among the heights of the Alps, the Pyrenees, and even the Himalayas. For the same reason *Carabus auratus*, which inhabits the plains of France, is not met with in Italy, except on the highest mountains.

These latter, it must be observed, have also their peculiar species, which are arranged in a certain gradation over these declivities after the manner of plants, but without the same regularity as these, in consequence of the power of locomotion which they all enjoy in a greater or less degree.

Finally, mountains, when they form continuous chains like the Andes in America, and the Himalaya in India, present an almost insuperable barrier to the diffusion of insects. Their locomotion being infinitely less powerful than that of mammiferous animals and birds, does not enable them to surmount such natural obstacles. It is thus that Mendoza, situate at the foot of the Andes on the east side, has scarcely a single species in common with Santiago in Chili, which lies under the same parallel, and is not fifty leagues distant in a direct line.

The running waters which traverse continents have but an inconsiderable influence on the progress of insects, since the largest of them, such as the Amazon, La Plata, and Mississippi, are by no means of sufficient breadth to produce an effect of this kind. If the species of the opposite banks are dissimilar, as sometimes happens, the explanation must be sought for in other causes, particularly those influencing the vegetation. At the same time it must be admitted that the aquatic insects of a country will be numerous in proportion to the abundance of its waters.

Influence of Organised Beings.—Certain mammiferæ and entire families of birds, live at the expense of insects. Their multiplication has consequently a powerful influence on that of the latter, and may even go so far as to put almost a complete stop to it, in localities of an insulated description. In the last century, the inhabitants of the Isle of France introduced a species of Alcedo or kingfisher, to destroy the locusts which ravaged their plantations, and these birds freed them from the evil in a short time. They had even a considerable effect on the entire entomology of the island.

Enemies not less numerous nor less formidable are to be found in their own class. Not only do the carnivorous species devour each other, but many of those which are phytophagous feed their larvæ with other insects. This is probably the reason, at least in part, of the preponderance of certain families over others. May we not, for example, to a certain degree, account for the multitudes of phytophagous coleoptera in intertropical countries, by referring to the limited number of carabidæ which exist there?

There is still another way in which insects exercise influence on each other, by modifying their stations. In our latitudes, the carabidæ, with very few exceptions, live on the surface of the ground. In equatorial America, on the contrary, the greater part live on trees. This appears to arise from the innumerable legions of ants which have taken possession of the soil, and constrained them to take refuge on vegetables.*

Finally, man himself is not without exercising a great influence on insects, both with regard to their habitation and stations. He has transported them either voluntarily or without his knowledge, to great distances; as has been the case, for example, with the common hive-bee, which he has introduced into the new continent, and the *Blatta Americana* which he has conveyed to Europe, where it is now completely acclimatised.* With regard to stations, he modifies them by changing the vegetation, the temperature, and other physical conditions. Of this we are comparatively insensible in countries like those of Europe, which have been cultivated from time immemorial, but it is striking in those which are covered with primitive forests. M. Lacordaire has observed, as a common occurrence both in Brazil and Cayenne, that wherever the natural wood was cut down for the purpose of forming a plantation, along with the new plants new insects appeared which were but very rarely seen in the surrounding forests. Thence in these countries, the researches of the entomologist instead of being retarded by the burning of the forests, are rendered more fruitful. If these clearings, however, are made on a very large scale, the contrary sometimes takes place. M. A. de Saint-Hilaire has noticed that insects have disappeared almost entirely from the plateaus of the province of Minas and Brazil, since they were deprived of wood, and overrun by a graminivorous parasite, the *Capia gordura*, which chokes all the other plants.

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* The cause here assigned by M. Lacordaire for the difference in question, scarcely seems satisfactory, for, even though adequate to produce the alleged effect, its operation must be confined to particular localities. It must doubtless be sought for less proximately in the benign ordination by which every animal is adapted to the circumstances in which it is destined to exist. In countries so densely clothed with vegetation, the expansive and almost continuous foliage forms the platform on which insects live, rather than the surface of the ground, which is frequently in deep shade, and therefore uncongenial to their nature. The habits of tropical carabidæ are therefore primarily and necessarily arboreal, because it is on trees that they find their appropriate food. No change of condition in the soil itself could change the habits of the carabi of northern latitudes so far as to make them frequent trees, because they are often apterous, and otherwise unfitted for such a mode of life.

Influence of Locomotion.—It is evident that the influence of this cause will be the more powerful in proportion as the species possess greater powers of flight, and that it will consequently operate chiefly on the diurnal Lepidoptera, the Hymenoptera, the Diptera, and a few Orthoptera, which are most remarkable in this respect. One of the most interesting examples that can be cited of the diffusion of a species from this cause, is that of the bees brought from Europe to North America. They have again become, as is well known, almost completely wild. According to M. Warden,* the common hive-bee was unknown, in 1797, to the west of the Mississippi. Fourteen years after, it had not only crossed that river, but had ascended its course, as well as that of the Missouri, to a distance of six hundred miles. It had thus advanced at the rate of forty-three miles yearly.

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* M. Lacordaire mentions the following as a curious instance of the manner in which insects are sometimes transported to great distances from their native haunts. In 1825, when on his way from Buenos Ayres to France, he found, when under the line, a living example of *monochamus sutor*, an insect of the north of Europe, and an inhabitant of Britain, clinging to the shrouds of the vessel. Shortly after, the sailors brought him several others which they had found in other places. The carpenter having occasion to cut a plank of fir, several monochami issued from their holes, to the amount of about a dozen. The insects had thus travelled from Norway to France, then from France to Buenos Ayres, where some of them must have been disclosed, but it is doubtful whether they could have been acclimatised there, as there are no pines. Many examples might be mentioned similar to this: *monochamus dentator*, a North American species, has been taken at Havre; *cordylocera nitidipenni*, a native of Senegal, near Paris; a *pyrophorus* (one of the luminous elateridæ) in the same city; and to mention no others, the huge bird-spider (*mygale avicularia*) at Rouen.

Causes of Stations and Habitats.—We now come to inquire whether stations and habitations can be explained by all the physical conditions which have been alluded to. With regard to stations, the case seems not at all doubtful. An insect in this resembles all other animals, that it can live in a locality only as long as it finds there the conditions of nourishment, temperature, light, &c. necessary to its existence, and as long as other organized beings do not drive it away. It will multiply the more abundantly within the limits assigned to its species, if these conditions are united in a high degree, and if some of the principal of them are withdrawn, it will either quit the locality in question or perish.

Still, when we come to the actual application, we often find it very difficult to explain certain diversities of station observed among species of the same genus, whose habits are entirely alike. For example, M. Humboldt has noticed (and M. Lacordaire has made the same observation in Guiana) that in the deep forests of the Orinoco, notwithstanding an entire similarity in the conditions of temperature, humidity, light, &c. the different species of *Maringouins* (or musquitoes) are stationed in very restricted localities, in so much that each canton and each river possesses its peculiar species which never leave it nor mingle with the neighbouring species. In such cases, there must be certain conditions which we know nothing of, and which we are ignorant where to look for.

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* **Essai statistique, politique, et historique, sur les Etats-unis d'Amerique, tom. iii. p. 139.**

It is easy to demonstrate that the causes above mentioned are of no account in producing the existing diversity in the habitations. In regard to that, one of two things must have taken place; either the species have been created simultaneously on different points of the globe, in which case the question resolves itself; or else, as the generality of naturalists formerly thought, they were created at one point whence they were spread over the rest of the globe. But if this had been the case, the species must have been otherwise distributed than we now see them; they would have been spread in the direction of the isothermal lines, and the same species would be found encircling the globe, following the flexuosities of these lines; if they became gradually modified as they receded from the point of departure, they would follow, step by step, these successive modifications. But we witness nothing of this kind, and consequently it is more than probable that, at the origin of things, Providence placed these animals in the places where we now see them, adapting each to the climate under which it was destined to live, and conferring upon it at the same time a sufficiently flexible organization to admit of its departing more or less from the centre of creation. Physical conditions would only have the effect of modifying slightly this primitive distribution.

Another question remains to be discussed, intimately connected with the preceding, namely, whether we ought to consider as derived from the same stock, individuals of certain species which are spread over the greater part of the earth, or which exist in countries widely remote from each other.

When two countries, however distant, are in communication by means of intermediate countries, and the insect species phytophagous, and of powerful flight, we may suppose that it has advanced insensibly into these countries, wherever it found the plant suited to it. It is thus that we can explain the dispersion of *Venussa cardui* through Europe, Asia, Africa, and New Holland, because, in the caterpillar state, it lives on the carduaceæ, malvaceæ, urticeæ, &c. families of plants which have all representatives in these countries. And these countries, besides, are in continuity with each other, except New Holland, which is, however, so near the Indian Archipelago, that it may receive a species from thence. But it becomes more difficult to explain its presence in America, where it likewise exists, especially in the United States. M. Lacordaire has likewise taken it in Cayenne and Brazil. However, we may still account for the fact by supposing that it has passed by the north from one continent to the other, or that the eggs have been transported along with the plants on which they were deposited. Numerous instances of such transportations occur among the Lepidoptera. Among these, that of *Nymphalis Bolina* may be cited, a species proper to Africa and equatorial India, but which is likewise found in Cayenne, whither it has, no doubt, been conveyed along with some of the numerous Asiatic and African vegetables acclimatised in that country.

We may admit the same cause in regard to certain creophagous species, which, in the larva and perfect state, live on dried animal substances, such as skins, and thus explain the existence of *Corynetes rufipes* in Europe, California, Buenos Ayres, and New Holland; that of *Dermestes vulpinus* in Europe and the greater part of America. But there are cases in which an explanation of this nature will not avail.

Thus, the *Prestonychus complanatus*, a species of southern Europe and the coast of Barbary, is found on the mountains in the neighbourhood of Valparaiso in Chili, and that is the only spot in America known to produce it. The most minute comparison fails to discover any difference between individuals taken in this locality and those of Europe, so that their specific identity cannot be questioned. But how can we explain the presence, in places so remote from each other, of a carabideous insect which is by no means common, which lives, as a larva, in the bosom of the earth, which never comes in contact with man, and which the latter consequently could not transport from one place to another; in this case it appears necessary to admit that there have been two primitive stocks, each of which have propagated themselves in their separate localities.

If a multiplied origin is rendered thus probable in regard to one species, analogy authorizes us to infer that the same thing holds with many others, even in cases where it is absolutely impossible to explain their distribution. This is, in some degree, a consequence of the creation of species on many points of the globe at once; for, if nature thus create different types at many points, why may she not have repeated in one place, a type which had been already produced in another? A final consideration may be derived from plants, in regard to many of which botanists acknowledge a multiplied origin; and, in the present case, inferences regarding the animal may be legitimately drawn from the vegetable kingdom.

(To be continued.)

Observations concerning the Milk of Cows, labouring under an Epidemic Disorder, called COCOTE, *together with general considerations concerning such matters as may injuriously affect the Animal Economy, and be discovered in the diseased products, or in the atmosphere or water.* Presented to the Academy of Sciences by the Chemical Section, M. CHEVREUL, Reporter.

During the past winter, 1838–9, the milch cows of Paris and its vicinity have very extensively been visited by one of those epidemics, which among our neighbours is recognised under the name of *epizootic*, and among ourselves as a *murrain*. The same species of complaint had not been witnessed in Paris since the year 1810, and the attack of the current year, though very general, affecting most of the cattle, was not of a very aggravated nature. Though the mortality was not great, it produced most marked, and often unpleasant effects upon the milk, and thus a set of the most interesting phenomena, concerning epidemics and their effects upon the frame, and especially on the secretions, were brought under review; and not only as affecting the lower animals, but also by an easy and natural transition, upon man himself. Dr Al. Donné led the way in these investigations. In the year 1837, he published a pamphlet *Upon Milk, and especially upon that* *of wet-nurses, and its regarded nutritive qualities, good and bad, and the alterations it underwent.* On the outbreaking among the milch kine, he undertook most minute chemical investigations into the changes effected on the milk of the animals labouring under the disease, and communicated the results to the French Academy. A commission was thereupon appointed, which still farther prosecuted the inquiry, dwelling moreover upon the effects produced by the use of the milk from the diseased cows, and more especially insisting upon the more general subject "*of those researches which ought to be undertaken, whereby chemistry would throw all possible light upon murrains, upon epidemics and contagious diseases.*" As the report of this commission extends far beyond our limits, and much of it bears upon the peculiarities of this epizootic, we shall pass at once to the more important and general discussion we have just announced.

When murrains, and still more when epidemics or contagious disorders which affect mankind occur, the serious losses which the former occasion among the animals which supply our daily food, or which we employ in labour, and the heart-rending desolation which the latter occasion among the crowded populations they attack, and the hidden mystery respecting the nature of those influences which threaten the existence of all who fall within their sphere, are so many urgent causes which induce us to seek for all the information we can possibly obtain. It is under these circumstances, that often much astonishment is expressed that chemistry can supply no response to the crowd of inquiries which are made; and the more so, that much is expected, as it is generally understood that this science can do much in penetrating into the intimate nature of all bodies. If this science is still incapable of answering to many of the questions which are suggested by the appearance of these dreadful scourges, it will be useful to point out the reasons of this inability, and afterwards to demonstrate what may be expected when the limits which now confine its operations shall have been extended.

The inquiries to which the science of chemistry should be able to respond, respect the nature of matters belonging to two very distinct categories, viz. 1*st*, Concerning the organic materials which go to the formation of animals, such as the blood, bile, milk, &c. and also the flesh, nervous matter, &c.; and, 2*d*, Concerning the nature of the materials of the external world which exert an influence upon the organised beings which are afflicted with the scourge. Such are the waters, which may be drunk; and, still more, the atmosphere, and such agents as it may hold suspended.

ARTICLE I.—*Question concerning the nature of organic matter.*

The knowledge of the disorders which are introduced into the animal economy, by the invasion of a disease, requires, to be at all scientific, that the physician should define the symptoms of the complaint; that the pathologist and physiologist should describe the lesions which have taken place in the organs of the individuals who have become its victims; and, finally, the chemist examines the solids, fluids, and excretory products, that he may point out the modifications they have undergone from the disease. Hence, *it is necessary to have direct analyses of all these matters in their healthy state*, that they may serve as TERMS OF COMPARISON *to the correspondent materials taken from those afflicted with sickness, and which ought to be submitted to analogous analyses.* These analyses, however, which must supply the very terms of comparison, and which imply an acquaintance with proceedings so precise, as to enable us not only to give an exact enumeration of the immediate constituent principles of the solids, liquids, and the various excretions, but also the relative proportions in which they are found—for it is clear, the difficulties will be great when the essential principles of life appear in different proportions from those which are required in the healthy state of the being they form—these analyses, we say, *we have not.* In fact, we have not yet reduced to precise formulæ the processes which are necessary for the exact determination of the immediate principles of the blood, bile, &c. so that a chemist perfectly master of the most recent scientific methods, can find no one prepared to his hand whereby at once to analyze a specimen of any substance which may have been precipitated; and, moreover, there is still much uncertainty concerning the properties which characterize certain proximate principles, whose influence in the animal economy is most important. As examples, we name caseum, fibrine, and the albumen which is united to a sulphurous compound, and with whose nature we are entirely ignorant. We may even add, that the direct quantitive analysis of milk has not advanced progressively with the experiments which have demonstrated that its *butyrin* is very complex, as it contains, in the milk of cows at least, besides the *stearin* and the elain (*oléine*), a substance which is much more soluble in alcohol than these latter, and which is probably formed of three different substances, *butyrin*, *caproin*, and *caprine*.

The effect of this state of matters will at once be seen by those who are at all acquainted with chemistry, namely, that without the direct analysis of the parts in health, and of precise formulæ whereby to execute them, when at any given time it is necessary to compare a diseased organic substance with a corresponding healthy one, we are destitute of the principal data, and of the very *terms of comparison;* and this difficulty of arriving at a satisfactory result is much increased by the small quantity of matter which is supplied to the chemist, and by the shortness of time during which he can procure even this small quantity, since the diseased substances are almost always changing their nature, and hence it may be impossible to verify an important induction suggested by the experiments made upon the last portion of the substance he had received.

We shall now advert to the difficulty of the direct normal analyses, and to the methods, the prosecution of which promise the greatest prospect of a favourable termination.

Were we to propose to an able chemist experimentally to undertake to establish the normal analysis of an animal liquid, such as the blood, or milk, we believe he would commence with the condition of maintaining the existence of every substance which he isolated, as he would do were he perating upon metals forming an alloy, or upon oxygenous compounds forming a stoney mass. We shall demonstrate this proposition by incontestible facts.

Previous to the time when the various bodies which form the chief mass of the fatty substances most commonly found in man and the domestic animals were accurately distinguished, a *fatty matter* was regarded as holding a place among the constituent principles of blood, bile, &c.; and it was stated that alcohol and ether, by reacting upon fibrin, tendons, and the tissue which, by boiling water, is transformed into gelatine, produced a *fatty matter* at the expense of the elements of these substances. But here this phrase, *fatty matter*, expressed nothing more than this, namely, *that there was found in the blood and in the bile, &c., and so in the alcohol and ether which had acted upon these animal substances, an inflammable matter which was insoluble in water, and soluble in alcohol and in ether.* Besides, the most able chemist who would have endeavoured precisely to have characterized these several fatty substances, without at the same time undertaking a course of experiments upon all the other substances besides those which were actually submitted to his analysis, would have thrown away his time, on account of the substances detected in the blood, bile, &c. being found in too insignificant proportions, and mixed up with so many foreign bodies, that it would have been impossible for him to arrive at any precise result. This end, however, has been attained without difficulty when, in studying the principal fatty substances, readily procurable in quantities sufficient for all imaginable experiments, we commence with the products of saponification, much more easily characterized than previous to that process. These last have been successfully defined and characterized; and fatty bodies have been reduced, 1st, into perfectly defined acids; 2dly, into neutral saponifiable bodies, that is to say, such as are reduced by alkalies into different well defined products; and 3dly, into neutral unsaponifiable bodies. It has thus been possible, and without much difficulty, to reduce the fatty matter of the blood into the fatty matter of the brain, and into margaritic and oleic acids, and cholesterine; and the fatty matter of the bile into cholesterine, and into margaritic, oleic, and other acids. It has also been easy to recognise the fatty matter of the brain in the fibrin of the blood, and stearine and oleine in those tissues which may be reduced to gelatine.

We conclude from these facts, that, in undertaking, with any hopes of success, a direct normal analysis, we must first be certain that we define, if not all the proximate principles of any given organic substance, at least the principal of them, respect being had to the importance of their properties, and the great proportion in which they are held; and if this certainty cannot be attained, and we do not recoil before the troublesome labour, we must then seek for those proximate principles which we judge to be the most analogous to those we wish to separate from the substances in which they are most abundant, most isolated, or in the state of easiest separation; so that we may study their properties, and select those essential characters which will be recognised with most facility, and which will serve as the distinguishing character whereby we may judge whether the separated principles of any substance of which we are desirous of making the normal analysis, are identical or only analogous to the first.

There is one consideration which ought not here to be omitted, viz., that there are sometimes globules, and these occasionally so small, that, simply suspended in animal fluids, they are perceptible only with the help of a lens or the microscope. In all cases, therefore, of the analysis of liquids, and even in that of those substances which appear homogeneous to the naked eye, it is quite indispensable to have recourse to optical instruments, that we may ascertain if there be globules in the fluid, and several distinct substances in the solids. If globules are present, they must be studied as bodies independent of the fluid in which they are discovered; and if *distinct parts are found in a solid*, they must, if possible, be separated mechanically; and when this is not practicable, we must have recourse to the simplest reagents, such as water, alcohol, ether, &c. Let us dwell for a moment on the mode in which the chemist should examine globules.

If, with the help of the microscope, we ascertain the form of the globules, and traits of an organic structure, if existing, or any alliances of a physical kind which they may possess, either among themselves or with another substance which is not globular, then *these globules, as matter, entering within the domain of chemistry*, ought to be studied in relation to their intimate composition, and under that of the elementary composition of their proximate principles, if resulting from the union of several of these principles; this study presupposes that they have been isolated from the liquid in which they have been disseminated, and, hitherto, it is but too true that but very feeble attempts have been made to succeed in any endeavours of this kind. Although it is confessed that difficulties present themselves to the isolation of the globules, we cannot too much insist upon the advantage it would be to the natural sciences were one and the same observer to devote his efforts to the comparative study of all the globules which are found in animal fluids, so that the analogies and the differences which exist among them might be ascertained. The experimenter should abstain from forming alliances of identity, which are based only on form which, even on account of its simplicity, may belong to very different kinds of bodies; and he ought never to forget that the identity in nature can never be demonstrated but where there is identity of chemical properties, among which we comprehend elementary composition. An observer might deduce many valuable conclusions from the use of reagents, and especially if he were careful not to confound them with the phenomena which might be produced by their action upon bodies dissolved in liquid in which globules existed..

From the preceding considerations, which have a direct relation to questions concerning the nature of organic substances in general, we pass to the study of milk in particular, which is the special object of this report, and may conclude,

1st, That not only is the analysis of healthy *(normal)* milk wanting, but even that the present analysis is not up to the level of the labours which have put us in possession of the immediate composition of butter, since it has been considered, in all the analyses which have been made since these successful labours, as a constant substance, and without taking any account of the respective proportions of the stearin, the olein, and the butyrin, and also of the caproine and caprine which form it.

2d, That caseum has not been sufficiently studied in relation to the other principles containing nitrogen, such as fibrin, albumen, coagulated albumen, so that, in the analysis of diseased milk, it is impossible to express a precise opinion concerning the modification which the caseum may have undergone from the disease.

3*d*, Nothing can more strikingly demonstrate how very vague our real knowledge of healthy milk now is, than the difficulty we have experienced, when we would, in this paper, state its action upon coloured agents. Macquer, in the *Dict. de Chimie*, informs us that the healthy milk of a frugivorous animal is neither acid nor alkaline, but neuter. M. Bouillon Lagrange in the *Ann. de Chimie*, tom. 50, states, that milk recently drawn reddens turnsol paper. M. Thenard, in the 59th volume of the same work, recognised the same property on its issuing from the mammary glands; and Thomson and Berzelius are of the same opinion. In a journey which Messrs Gay Lussac and Darcet made in Belgium in the year 1826, they found the milk alkaline, when drawn from the cow, in forty different animals. M. Payen observed the alkaline character of woman's milk in a number of instances, and its neutral state in the goat. (*Journal de Chimie Medicale* 1828.) M. Lassaigne (*Ib.* 1832) having examined the milk of a Swiss cow two and twenty days before calving, found the milk, or rather the liquid which represented it, alkaline; but, eleven days after this trial, the milk had become acid, and this character continued after it had calved. Finally, and more recently, M. Piligot and M. Lassaigne have considered acidity as a property of healthy milk.

We have considered this difference of opinion upon a fact which may easily be determined, was a powerful motive to induce us to add some new observations to those we have just mentioned. One of the members of the Commission accordingly went to the Veterinary School at Alfort, where M. Lassaigne had the kindness to put him in circumstances which enabled him to establish the following facts. The milk belonging to three English cows, of which one had calved eight months before, and the other ten, when drawn from the udder and applied immediately to paper reddened with turnsol, made it blue; *it was therefore alkaline.* The milk of two goats, whose kids were ten months old at the time of the experiment, was alkaline. Finally, two Merino sheep of pure breed, taken from a flock of forty ewes, the one of which had dropped her lamb two months before, and the other three, supplied milk which also was alkaline. Here, then, we have seven specimens of milk, taken from seven individuals belonging to three different species of animals, all of which were alkaline. Does it therefore follow that milk always possesses this property in its healthy state? We should be tempted to draw this absolute conclusion from our own observations, and from those analogous ones formerly made, which we have cited; but, on the other hand, when we remember that chemists, celebrated for their accuracy, have stated *that they have established the neutral state, and even the acidity of milk when first drawn*; and that M. Lassaigne has found in the same animal, whilst subject to a constant regimen of nourishment, that the milk was first alkaline, and after a time became acid, the Commission cannot consider that it has settled the question. It will therefore only say *that healthy milk* appears *to be usually slightly alkaline, and that future observers must still determine if there be circumstances in which the milk becomes acid, without it being possible to recognise in the animal which produces it, the slightest symptom of disease.* It will be understood that we speak only of milk at the moment it is drawn from an animal in health; for we know that disease or any sudden agitation has made the milk acid.

4*th*, To those difficulties which arrest the chemist in the accurate study of milk in its healthy and diseased state, and on which we have already dwelt, we must add another, which arises from a want of precise knowledge of the true composition of the morbid products which may be mixed with the milk in the mammary organs; of which pus is a good example. If the observation of the morbid state of the organs in which it is found, enables the physician to apply this application without a doubt, it is very different with the chemist who shall be required to recognise and demonstrate the presence of pus in blood, milk, &c., because, for an indisputable demonstration, he must neither confine himself to a microscopic observation, nor to the phenomena produced by reagents upon substances whose minute nature is indetermined. In fact, it is not sufficient to have determined by the microscope a difference between the globules of a healthy fluid, and those of this same fluid which has been mingled with pus, that we may thereby be in a condition to supply the demonstration of which we are speaking. For, until we have given a *specific character* to the globules of pus,—until we have defined the immediate principles which constitute it, and the characters of these principles,—until we have defined all the substances which may produce pus,—and the circumstances which, by acting upon some of its principles, cause it to become putrid, it will be impossible to solve with certainty the question which we have proposed in general terms. To justify this conclusion, we have only to state, that *colostrum* (an ingredient of many milks for some time after parturition) has the property, according to M. Donné, of thickening the milk of which it forms a part, on the addition of ammonia, as much as pus itself.

ARTICLE II.—*Concerning the nature of those substances of the external world which exert an influence over organized beings.*

The atmosphere has such a vast influence upon the existence of animals, that, in all times, mankind have been led to suspect that it was the cause of many maladies which at once attacked a great number of individuals. It was on account of this opinion, that, at the time when oxygen and nitrogen were discovered to be the essential elements of the atmosphere, the name *eudiometer* was invented to designate the instruments intended for detecting the respective proportions in which they existed, and, by extension, the presence of bodies which might be accidentally mixed up with them. Those researches, however, which have hitherto been made, with the object of discovering in an atmosphere, where the population has been struck with an epidemic disorder, some matter which might be the cause of that malady, have had no precise result; either, as has been asserted, by the discovery of some peculiar foreign matter; or the very reverse, as has been alleged, by no difference between the air of this atmosphere and wholesome or normal air, being recognised. We shall dwell, for an instant, on these two cases severally.

1st case.—If we have recognised a compound of carbon and hydrogen in an atmosphere which is alleged to be vitiated, by means of some absorbing reagent,—or, if we have concluded that we had found a poisonous miasma, because the water, precipitated by some means or other from this atmosphere, presented phenomena which result from the spontaneous decomposition of organic substances,—we have not established the conclusion in question by experiment, which should have consisted of a demonstration of the deleterious properties of the two substances. This demonstration is absolutely necessary; for we have only to remember—that oils and empyreumatic acids are unceasingly mixing with the atmosphere in consequence of incomplete combustion,—that carbureted hydrogen is ever developing itself from morasses,—and that volatile organic substances, such as essences, aromas, &c. proceed from vegetables and animals,—to be convinced, that, by submitting a sufficient volume of air to the processes of precipitation, we may manifest in ordinary wholesome air, the existence both of organic matter and of carbureted hydrogen.

2d case.—On the other hand, in the case where the presence of a miasma or some deleterious substance of animal origin in the atmosphere is denied, because, by eudiometric methods, we cannot shew it contains any substance which is foreign to the wholesome or normal state of the atmosphere, our conclusion is far too rash. For there may be in an atmosphere some deleterious substance, which shall escape the acumen of the chemist, inasmuch as it is in too small a proportion to the normal air to be recognised by means of reagents, precisely as the presence of a metal, which had not yet been described, would escape the notice of the most able analysts, if this metal was contained in an alloy, but in extremely small quantity. We ought here also to remark, that if chemical analysis should not demonstrate the existence of this foreign body in the air, which accidentally contains a substance sensible to one of our senses, this is not always a reason for our concluding that it is incapable of doing so, and even by means of our present methods; for it may be possible in two ways, 1st, by the help of some mechanical or physical method: and 2dly, by the more usual means; but where, instead of seeking for the deleterious substance in the atmosphere, in which it is contained only in small quantity, it will be discovered in some solid or liquid substance, which will furnish it in quantities sufficient for extensive examination.

A. *By the help of some mechanical or physical method.*—We can conceive the possibility of effecting the liquefaction or the solidification of a miasma which was in a state of vapour in the atmosphere, either by compression or by refrigeration. The deleterious matter thus liquefied or solidified might ultimately be studied by means of our present chemical processes, in the same way as hydrocyanic acid, morphia, picrotoxia, strichnia, &c. have been; and then we should no longer be compelled to admit that miasmata escape all chemical analysis, or that they are imponderable fluids. Nor would the result, whose possibility we now contemplate, be altogether new in a scientific point of view, since Dr Faraday has already examined the carburetic hydrogens which have been separated by great compression from the common illuminating gas. This instance has pre-eminently the advantage of enabling us to perceive how the perfecting of purely mechanical or physical methods, which condense or refrigerate the gas, and collect the liquid or solid products of the condensation, may contribute to the advancement of the sciences of chemistry, physiology, and medicine.

B. *By the assistance of the present methods.* Chemistry may succeed in recognising the nature of a miasm, by detecting it, not in the atmosphere, but by separating it from some solid or liquid substance, or by determining its formation at the expense of the proper elements of that substance; and the proof that this supposition is not unreasonable will be found in the following facts. If, before the discovery of the volatile acids, to which butter is indebted for its persistent and characteristic odour, it had been proposed to a chemist, that he should discover the nature of the odorous substance which pervaded several pints of air, after having been for 24 hours in contact with butter, to a certainty he would have failed in all attempts, and chiefly on account of the insignificant quantity of the odorous substance. In spite, however, of this, no sooner did searching investigation into the products of the saponification of butter make us acquainted with butyric, caproic, and capric acids, than the question concerning the aromas which butter imparts to the air is actually solved. Starting, then, from these facts, let us suppose that the air charged with the odour of butter, which only affects our sense of smell, was deleterious to an animal, and it will then be manifest, that chemistry, which was incapable of discovering this miasm in the air, would have succeeded in recognising it, by studying the substance which produced it; and this is an example, quite in point, demonstrating that careful research may shed an unexpected light upon a subject which seems absolutely foreign to that of the researches more especially under review.

The waters which are used for drinking suggest considerations perfectly analogous to those we have been contemplating respecting the atmosphere. For, in many instances, it cannot be doubted that they do not merit the careful attention of chemists, not only on account of the very minute quantities of any active ingredient which they contain, but also on account of the atmospheric oxygen of which they may be more or less deprived, or that they have so far been deprived of the contact of air that they could not absorb enough for saturation, or because they may have been mixed with some combustible organic matters, which would appropriate the oxygen they had imbibed from the surrounding atmosphere. At the same time, the great use now made in many of the arts of poisonous compounds, such as the salts of arsenic, copper, &c. ought to arouse attention; for it is possible that the waters which have been employed in washing stuffs impregnated with arsenical compositions may, in some localities, have an injurious influence upon animals. It is also possible that the same effect may be produced by those substances containing arsenic, which have been buried in the earth, and which, disseminated by subterranean waters, may appear at the surface of the soil, far from the places in which they were originally deposited.

Consequences which may be deduced from the foregoing considerations.

Inquiry concerning principles exerting an active agency upon the animal economy which may be found in the atmosphere, in water, &c. or which may result from a change of the equilibrium in the elements which constitute organic substances, whether this change occur in what is called *fermentation*, or in the putrefaction of matter which belonged to a living being, or which may take place in an individual labouring under disease, should be investigated as among the most important researches connected with the history of the animal economy. If at the present time the chemist is not called upon to express his sentiments respecting the definitions of the causes to which endemic, epidemic, contagious and infectious disorders are attributed; and if in the physico-chemical sciences, he discovers reasons for studying the influence which the wind and sudden changes of pressure produce, also those which the temperature and humidity of the atmosphere may effect upon the animal frame, these are the strongest possible motives to excite to researches, tending to discover the causes of a disease which is destroying a population, in some deleterious substance which may be spread throughout the atmosphere, or contained in the waters, or in some morbid products. He ought not, therefore, to be inclined to adopt the opinion of some individuals who have been in too much haste to conclude affirmatively that there are no such substances, either as deleterious effluvia, miasmata, or noxious virus, because all the researches which have been undertaken to discover their existence have hitherto given only a negative result; and, on the other hand, where he does discover some particular substance which he suspects had a deleterious influence, and which, on further inquiry, he found had not so in reality, he must, ere his researches are complete, advance to new investigations respecting the animal economy, by employing, not only the matter in question but the products also which he may procure from it, under the influence of air, water, heat, &c. For example, let us suppose that butyric acid was a miasma or poison to some animal, it is evident that were the butter destitute of the acid, it would be innocuous to it, and that if, from the action of the atmosphere, or any other cause, it afresh disengaged butyric acid, it would hereby become deleterious. Nothing on this subject is of more importance to the chemist than the solution of the problem, which may be stated in these terms—*An effect being given, to know the specific nature of the bodies which produce it, and the circumstances in which they manifest themselves to our observation.* There are problems, belonging to this general proposition which have been solved, when a compound substance, endowed with strong influence upon the animal economy, such as opium, cinchona, nux vomica, &c. being given, Messrs Sertuerner, Duncan, Boullai, Pelletier, Caventou, &c. have extracted from them their active principles. These discoveries have so much of a chemical character about them, that the spirit which made them cannot but direct the efforts of analysis when applied to the inquiry concerning the deleterious substances which analogy leads us to presume exist in the atmosphere, in water, and morbid secretions, &c. In recommending investigations of this sort, however, we cannot too strongly insist upon the critical accuracy which must be maintained throughout. It is not because a peculiar substance has been found in an atmosphere which has been suspected to be vitiated, or in water which is supposed to be prejudicial to health, or that a peculiar proximate principle has been recognised in morbific products, that we should attribute to this body or principle, the cause we are in search of. Such a conclusion can be allowed only where it shall have been proved by a positive experiment that the effect whose cause we wish to discover, is the result of the mutual action of this body, and of a substance belonging to the animal economy; for it should never be forgotten that often in the animal machine a morbid principle, that is to say, a principle whose elements have been associated under the influence of some disease, may be perfectly harmless upon the animal frame, as is the saccharine matter of Diabetes. Hence the presence of this principle in the morbid matter which contains it, can only be an index, a symptom, and not the cause of this disorder; and it is under this last point of view that the agglomerated mulberry and mucous globules should be considered in the milk of cows attacked with the *cocote,* provided the probability we now possess of the harmlessness of this milk becomes a certainty, by subsequent researches.

Upon the Respiration of Plants. [*] By Messrs Edwards and Colin.

In the interesting science of vegetable physiology few or no facts are to be found more beautiful than those connected with the respiration of plants. The same remark, however, can by no means be made concerning the theory which combines these facts, and undertakes to explain them. In fact, we have always experienced the greatest difficulty in admitting this theory, whether considered in relation to the respiration of the seed or of the leaves: and, in truth, scarcely any other phenomenon has been recognised in the respiration of the seed than the disengagement of carbonic acid. This is usually explained by the combination of the oxygen of the air with the carbon of the seed. According to this view, the seed is supposed to be affected only by the atmosphere; the influence of water in this vital act of plants is considered either as absolutely nothing, or is limited to that of preparation and promotion, and it is held in no way to contribute to the production of the gas which is disengaged. This, then, is the first difficulty respecting this theory of germination which presents itself; and those which occur in regard to the explanation of the respiration of the leaves are still more serious. During the night, it is said carbonic acid is disengaged, whilst during the day it is absorbed, and oxygen appears under the direct rays of the sun. Here, then, are the facts, and here the explanation which is afforded: the absorbed carbonic acid must be decomposed by the plant, which again must appropriate the carbon, and disengage the oxygen.

But this capability of decomposing carbonic acid is conferring upon the plant a power which it is very difficult to admit; and it is very seldom found in the mineral kingdom, where the very great simplicity of the composition of bodies increases their decomposing power, and where the much greater number of elements, scattered throughout the different compounds of this kingdom, renders it probable that we should discover some endowed with this property. Finally, water, according to this supposition, is of little or no use in this action, although it is absolutely required for plants, and we are perfectly ignorant of the part it plays. These are some of the considerations which have induced us to undertake the examination of this function in plants; to which we were also conducted by some facts brought under our notice in agricultural physiology, to which we shall solicit attention in the sequel.

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[*] Read to the Academy of Sciences, Nov. 1838. From Annales des Sciences Naturelles, for December 1838.

Up to the present period, experiments upon the respiration of seeds have always been made in the air; or if made in water, the phenomena which occurred in the liquid have been limited to the explication of what took place in the air; the gas disengaged in the liquid has not been examined, and consequently its proportion has not been determined. What follows is an account of what we have done in relation to this point, and which has yielded very extraordinary results. Our operations were conducted on a great scale, that the effects of the experiments might be more distinctly brought out.

On this account, we selected for our operations a great ball-shaped bottle with a narrow neck, capable of containing six or eight pints of water. We filled this bottle, and introduced forty garden beans of a large size, without any fissure in the husk, or any other defect whatever. To this great bottle we adapted a bent tube, also filled with water, which finally was introduced into a receiver full of the same liquid.

By this arrangement, the beans were in contact only with water, and with the air which it contained, air which, under the circumstances, could not be removed; and this was one of those important circumstances which led to all the success of the experiments. The first phenomenon which presented itself was the disengagement of air-bubbles, which proceeded from the beans. These bubbles were at first very minute, insensibly they increased in size, and in the space of twenty-four hours they were very conspicuous.

This evolution of gas was itself a very extraordinary circumstance which had not previously been pointed out, and which scarcely seemed to agree with the received ideas upon germination; still less with the supposition that this disengagement proceeded from air contained in the beans; which idea soon became wholly improbable from the unceasing continuance of the disengagement of the gas, and to such an extent that it could not by possibility be attributed to this cause.

First of all, it is certain that the gas came from the beans themselves, for before we introduced them into the apparatus, we were careful to put them in water and shake them well, thereby detaching all the air which was attached to their surface. For a long time after they were plunged into the water of the bottle, no gas was seen to issue from their surface, and it made its appearance only by degrees. Besides, on other occasions when the beans were cut through, we have seen it proceed from the parenchymatous substance itself. Many of the beans were actually made to float by the air-bubbles which adhered to them, and fell to the bottom so soon as the bubbles burst.

After a period, which was never less than four days, we put an end to the experiment. Our first care was to weigh the beans, that we might thereby determine the quantity of water which they had absorbed, and we invariably found that it exceeded their own weight. In reality, the mean weight of the beans employed was 1540 grains (avoirdupois), and the moisture which swelled them amounted to about 1848 grains.

The most essential point of all in this investigation was to certify that the beans were alive, and in a state of germination; for it is evident that it is a condition which must indispensably be established, that the disengagement of the gas which is effected in the water is the result of a natural and normal function.

When taken out of the liquid, some of the beans had a chink opposite to the point where the radical is situate; but there were only three or four in this condition.

If the beans were living, the function was normal: so we planted them, that we might have an opportunity of comparing them with the same number of other beans which had not been subjected to any experiment, and we had the pleasure to see them spring up quite as well as these. The best method, however, of conducting the experiment is to keep them in moistened paper between two plates. Next day, during summer, they had all completely germinated, and the radicles had projected some four or five lines.

As to the production of the gas, we shall observe that that which was disengaged, traversed the water, and passed through the tube into the receiver, was only the sign of the function; and nothing more than the overplus of that which was dissolved in the water at the moment it was formed; it must also have been in very small quantity. The proportion of air which had traversed the water without being dissolved by it, amounted to between 20 and 40 millilitres; but that which was dissolved in the water, and which was disengaged by boiling, was very considerable, and might well, as it did, surprise us.

The whole interest of the experiment here depends upon the quantity of the air naturally contained in the water, compared with that which had been produced by the seed. Accordingly, we made many experiments to ascertain the proportion of air contained in the water of the well which we used; and we found that the water in our bottles before the experiments, as the mean of our observations, contained 7.5 centilitres of air, and after the experiment 55.5 centilitres, in an experiment of five days' continuance. Hence, after subtracting the air naturally contained in the water, we find 47.7 centilitres of gas, produced solely by the action of the water and the beans. The result of another experiment, which lasted for six days, after making the same subtraction, was 50.5 centilitres of gas, produced above the quantity of air naturally dissolved in the water of the bottle. There was, therefore, disengaged by the sole action of the seeds and the water, after subtracting the air which the latter contained, more than half a pint of gas; a very remarkable effect, and which, when seen on so great a scale, leaves not the slightest doubt as to the action of water in the respiration of beans, abstraction being made for the air contained in the liquid.

Our next object was to ascertain what information analysis would give us respecting the nature of the gas supplied by the seeds. And first, there was an enormous proportion of carbonic acid. Of the 55 centilitres produced by the experiment of five days' continuance in summer, 48 were carbonic acid. 2dly, An infinitely small quantity, 2.5 millilitres was oxygen, and about 3.5 centilitres, which appeared to be nitrogen. In short, there was, 1st, an enormous quantity of carbonic acid; 2dly, scarcely any oxygen; and, 3dly, a quantity of gas, which, in the mean while, we shall regard as formed entirely of nitrogen, and which amounted to somewhat less than the quantity of air contained in the water. On another occasion, we shall consider whether any other gas was present.

Whence, then, proceeded this enormous quantity of carbonic acid, in the production of which the air contained in the water must be counted for nothing? It is clear, that since the oxygen does not proceed from the air dissolved in the water, it must be derived from one of the elements of the water itself. The water, therefore, is decomposed; the oxygen, one of its elements, unites itself to the carbon of the seed, and forms the carbonic acid which disengages itself in whole or in part; a question into which we shall enter upon another opportunity.

It now remains to inquire what becomes of the hydrogen, the other element of the water? We suppose, for the moment, that there is no trace of it, as we have stated provisionally above; and since it is not disengaged, it must evidently be absorbed by the seed.

Hence, the results which follow from the experiments which we have detailed, from seeds placed in the conditions stated, are, 1st, that the water is decomposed; 2d, that the oxygen of the decomposed portion, unites with the carbon of the seed, and forms carbonic acid gas; 3d, that this carbonic acid disengages itself from the seed, in whole or in part; and 4thly, the other portion of the decomposed water, the hydrogen, is absorbed by the seed, in whole or in part. These are the four fundamental propositions regarding the respiration of seeds, to which we shall confine ourselves on the present occasion.

It is not a matter of very great moment to ascertain whether all the carbonic acid is completely disengaged. Nor is it of more consequence that we should know at present if all the hydrogen, rendered free by the decomposition of the water, is completely absorbed by the seed; subjects which, however, we shall discuss on another occasion. The great fundamental fact brought out by these researches is the decomposition of water, a fact quite foreign to the popular theory of the present day.

It also results from the facts which we have propounded, that respiration is not, as it has hitherto been considered, solely a function of excretion; but it at the same time exhibits, according as we have demonstrated, a fundamental fact concerning the nutrition and the development of the embryo by the absorption of hydrogen.

In addition to the respiration of seeds, a great variety of which we have examined, we have also investigated that of bulbs, twigs, leaves, and flowers, the results of which we hope to have the honour of presenting to the Academy. We may, however, remark, that the facts detailed in this memoir, regarding the respiration of the seed, form the basis of the respiration of other parts of the plant, as will be more clearly exhibited in the sequel, as well as the part which the air plays in this important function.

Elementary Considerations of some Principles in the Construction of Buildings designed to accommodate Spectators and Auditors. By John Scott Russell, M. A., F.R.S.E., &c.[*] Communicated by the Society of Arts.

This is a subject on which we are so notoriously deficient and so completely abandoned to chance or empiricism, that any attempt to approximate to normal systems of construction are likely to meet with more indulgent consideration than their own merits might, on any other subject, be able to secure. It is with this conviction that an unprofessional writer has hazarded the treatment of a somewhat difficult subject. As, however, the writer's ordinary duties require from him the daily use of buildings, to which the principles he is now to explain are applicable, he conceives that some circumstances may have been forced upon his attention from which important consequences may hereafter be deduced.

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[*] Read before the Society of Arts for Scotland 16th May 1838.

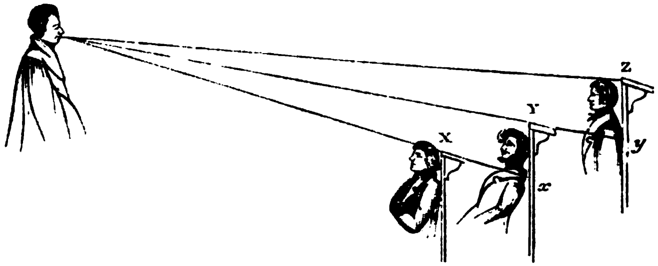
In almost every large room designed for an audience or spectators, or for both, because most people like to see, as well as to hear a speaker, it may be noticed that certain seats are *the best.* These seats are neither too far forward nor too far back, that is, they are not so far forward as, by being immediately under the speaker, to require to look up at a painful angle of elevation, and to permit his voice to pass over our heads; or, on the other hand, so distant as to throw us behind a mass of people by whom vision would be intercepted, and over whose heads we should require to strain either to see or to hear clearly. A perfectly good seat is one in which, without uneasy elevation of the head or eye, without straining or stretching, we can calmly and quietly take any easy position, or variety of positions, which we may be disposed to assume, and yet may in all of them see and hear the speaker with equal clearness and repose, so as to give him patient and undisturbed attention. The person who occupies such a seat feels as if the speaker were speaking principally to and for him; he finds that no one else stands in his way, and that he hears as well, and sees as well, as if there were no one else in the room but himself and the speaker. A room so constructed that every man in it should feel in this manner, that he had got one of the best places, and that no one else was in his way,—such a room would be perfect. Such a room, or rather approximations to such a room, we have sometimes, but very rarely, met with. On taking a particular seat, whether near the front or near the back, of the audience, we have felt the comfortable assurance of having one of the best seats in the room.

The object of this paper is to discover in what manner the interior of a building for public speaking should be formed, so that throughout the whole range which the voice of a man is capable of filling, each individual should see and hear without interruption from any of the rest of the audience, with equal comfort, in an easy posture, and as clearly as if no other individual auditor or spectator were present. (See Plates I. and II.)

The position of the seats is the first circumstance we shall investigate. Thats eats should slope, or, in other words, that the more distant auditors should be raised above those who are nearer, is at once seen; the question is how, and how much?

To see and hear comfortably, it must not be necessary for us to remain long in one position; we must not require to sit upright at full stretch, or to stoop on one side to catch a glance between an avenue of heads or hats; we must be permitted, whether tall or short, to sit in a comfortable variety of attitudes, now a little back on the seat, then a little forward, now stooping down, and now raised upright. In the usual variety of station and of position, it appears from experiments we have made that the range required for this purpose is more than a foot and less than 18 inches, so that these may be taken as the limits; that is to say, over the head of the person who is before you, there must be a clear range of 12 or 18 inches, through which the head may be moved upwards or downwards without interruption. In other words, as the undulations, both soniferous and luminiferous, move in straight lines, it is necessary for the purpose, that the rays of light and of sound emanating from the speaker, may fall, without interruption, on the organs of sight and hearing of the spectators and auditors; that a straight line drawn from the speaker's head over that of the anterior spectator, shall intercept the straight line which forms the back of the seat of the posterior observer, so as to cut off a height of 12 to 18 inches, within which the head of the spectator shall at all times be comprehended while sitting in a comfortable position. Thus let A be the speaker, and XYZ be three successive ascents; then

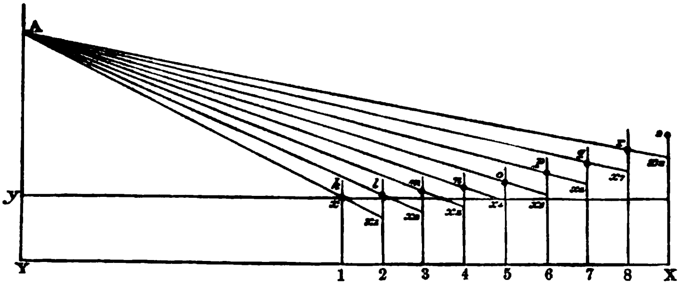
Fig 1.



the line AX must fall below AY, so as to leave the space Yx = 18 inches = Zy.

Let us now apply this formula to every individual place in the room or interior of the building, and we shall have the form required to satisfy the auditors. Let us also assume $2\frac{1}{2}$ feet as constant, and as representing the distance of one spectator behind the other measured horizontally, and $1\frac{1}{4}$ feet as the clear space, measured on the vertical line for the mean range of comfortable vision for each. If the level of the floor, that is of the lowest seats, be already determined, the form of the interior accommodation may be thus described. AY, the height of the speaker; YX, the level floor. From Ay take Yy = 4 feet.

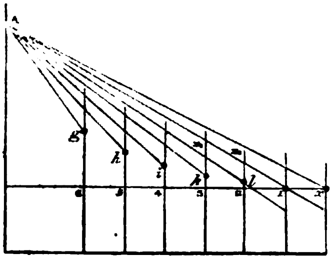
Fig. 2.



Draw yx parallel to YX. Take Ay to yx as $1\frac{1}{4}$ to $2\frac{1}{2}$, that is, as h, the range of position of the spectator, to d the distance between the seats. Take horizontal distances 1, 2, 3, 4, &c. = $2\frac{1}{2}$ feet; prolong Ax to x_1, then the height x_1 to $l = 1\frac{1}{4}$ feet. Join Al and prolong it to x_2, and take a distance x_2 to $m = 1\frac{1}{4}$; through m draw Am, and prolong it x_3, and take $x_3\,n = 1\frac{1}{4}$ feet; through n draw An, prolong it to x_4, and take $x_4\,o = 1\frac{1}{4}$ feet. Continue this process on in the same manner to p, q, r, s, t, &c., and you will find points which are the successive places which the heads of the auditors should occupy. The bottom of the seats must rise by the same quantity.

But it is not only in receding that the back seats must rise; we have already noticed that the seats which are too far forward are also unpleasant,—they are too low; they also should be raised, but this must be done so as not to interrupt those who are behind. This, too, may be accomplished in a similar way; for, as formerly set off, 1, 2, 3, 4, 5, 6, &c. = $2\frac{1}{2}$ feet. 1 is the

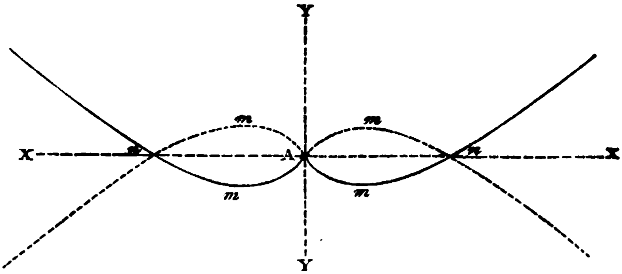
Fig. 3.



first anterior point; join Al, and let it cut the vertical line through 2 in x_2 the portion downwards $x_2 l = 1\frac{1}{4}$ feet; then l is the point found. Join Al; make $x_3\,k = 1\frac{1}{4}$; join Ak, and make $x_4\,i = 1\frac{1}{4}$; join Ai, and make $x_5\,h = 1\frac{1}{4}$, and so on; *f*, *g*, *h*, *i*, *k*, *l*, are the places found which the heads of the spectators will occupy, and shew the elevation to be given to the seats successively.

If the simple process we have described be accurately performed, the points which indicate the place of the spectators will lie in the branches of a very beautiful curve which may be termed *the equal-seeing or equal-hearing curve, the iseidomal or isacoustic curve.* This curve will be of the annexed

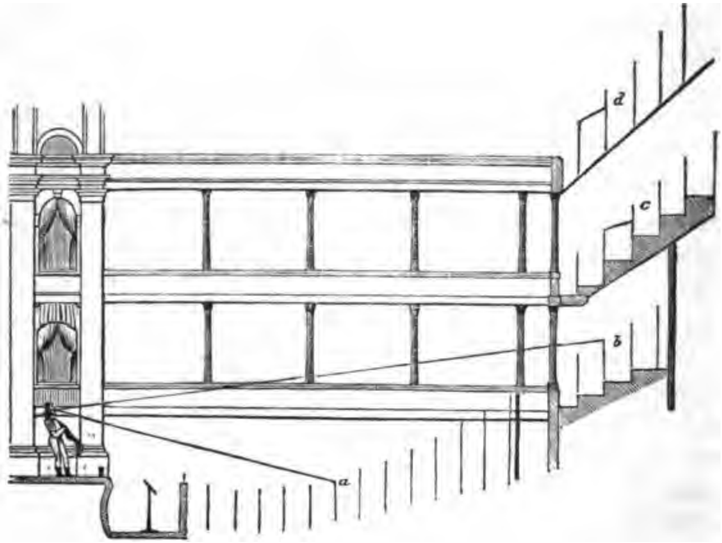
Fig. 4.



form;—A being the place of the speaker, and the heads of the spectators being placed on the line A m n continued as far as the voice of a man can reach, XAX being the axis of the curve and A y its parameter.

This curve has two branches on opposite sides of A, shewing that if the building extend behind the speaker, or if the spectacle be visible or the sound audible on every side, the same may be continued all round. From the property of this curve it follows, that if two straight lines be drawn to any pairs of points of it that are at equal horizontal distances, these lines will intercept the same parts of the vertical lines through these points. By means of this curve the position of seats in a theatre may be satisfactorily determined. Let A be the place of a figure on the proscenium, and the lines *a*, *b*, *c*, and *d*, the

Fig. 5.



vertical elevations of individuals in different parts of the house above the intercepted ray from A: then these being made every where equal, all parties see and hear without interruption. The evils arising from reflection of the voice from walls, and the evil of echoes, would be removed, if, by the elevation given to the hinder seats on this principle, the great proportion of the house-walls of the building were covered with auditors, and the sphere of sound and sight almost wholly occupied in useful effect.

For any great assemblage where it is desirable that one individual or group of individuals should be seen or heard, an amphitheatre of this form might be constructed from the surface of revolution, generated by moving the curve round its axis, which would perfectly accommodate 10,000 individuals. We have given, see Plates I. and II., a sketch of the interior of a lecture-room, of a church, or music-room, in which the auditors are placed in the isacoustic curve. The intelligent practical architect will readily see the method of carrying these arrangements into effect in other circumstances.

Researches in Embryology. Second Series. By MARTIN BARRY, M.D., F.R.S.E., Member of the Wernerian Natural History Society, Fellow of the Royal College of Physicians in Edinburgh.

The author having, in the *first series* of these researches, noticed at pp. 203, 4, and 5, of volume xxvi. of this Journal, investigated the formation of the mammiferous ovum, describes, in a *second series*, read before the Royal Society of London, its incipient development. The knowledge at present supposed to be possessed of the early stages in the development of that ovum, consists chiefly of inferences from observations made on the ovum of the bird.

But there exists a period in the history of the ovum of the mammal, regarding which we have hitherto scarcely any direct or positive knowledge. It appeared, therefore, highly desirable to obtain a series of observations in continuous succession on the earliest stages of development. In conducting this investigation, the author purposely confined his attention to a single species, namely, the rabbit, of which he examined more than a hundred individual animals. Besides ova met with in the ovary, apparently impregnated, and destined to be discharged from that organ, he has seen upwards of three hundred ova in the Fallopian tube and uterus; very few of the latter exceeding half a line in their diameter. The results of these investigations have compelled the author to express his dissent from some of the leading doctrines of embryology, which at present prevail, as respects not only the class Mammalia, but the animal kingdom at large. The following are the principal facts which the author has observed in the development of the mammiferous ovum.

The difference between the mature and immature ovum consists in the condition of the yelk; the yelk of the mature ovum containing no oil-like globules. Both maceration and incipient absorption produce changes in the unimpregnated ovum, which, in some respects, resemble those referable to impregnation. During the rut, the number of Graafian vesicles appearing to become prepared for discharging their ova, exceeds the number of those which actually discharge them. Ova of the rabbit, which are destined to be developed, are, in most instances, discharged from the ovary in the course of nine or ten hours *post coitum;* and they are all discharged about the same time.

There is no condition of the ovum uniform in all respects which can be pointed out as the particular state in which it is discharged from the ovary; but its condition is in several respects very different from that of the mature ovum *ante coitum.* Among the changes occurring in the ovum before it leaves the ovary, are the following, viz. the germinal spot, previously on the inner surface, passes to the centre of the germinal vesicle; the germinal vesicle, previously at the surface, returns to the centre of the yolk; and the membrane investing the yelk, previously extremely thin, suddenly thickens. Such changes render it highly probable that the ovary is the usual seat of impregnation. The author considers this view as being not incompatible with the doctrine that contact between the seminal fluid and the ovum is essential to impregnation, since he has found, in the course of his researches, that spermatozoa penetrate as far as to the surface of the ovary. The retinacula and tunica granulosa are the parts acted upon by the *vis a tergo*, which expels the ovum from the ovary. These parts are discharged with the ovum, render its escape gradual, probably facilitate its passage into the Fallopian tube, and appear to be the bearers of fluid for the immediate imbibition of the ovum. After the discharge of the ovum from the ovary, the ovisac is obtainable free from the vascular covering, which, together with the ovisac, had constituted the Graafian vesicle. It is the vascular covering of the ovisac which becomes the corpus luteum. Many ova, both mature and immature, disappear at this time by absorption. In some animals minute ovisacs are found in the infundibulum, the discharge of which from the ovary appears referable to the rupture of large Graafian vesicles, in the parietes or neighbourhood of which those ovisacs had been situated.

The diameter of the rabbit's ovum, when it leaves the ovary, does not generally exceed the 135th part of an inch, and in some instances it is still smaller. The ovum enters the uterus in a state very different from that in which it leaves the ovary; hence the opinion that "in their passage through the tube the ova of Mammalia undergo scarcely any metamorphosis at all," is erroneous. Among the changes taking place in the ovum during its passage through the Fallopian tube are the following, viz. 1. An outer membrane, the chorion, becomes visible. 2. The membrane originally investing the yelk, which had suddenly thickened, disappears by liquefaction, so that the yelk is now immediately surrounded by the thick transparent membrane of the ovarian ovum. 3. In the centre of the yelk, that is, in the situation to which the germinal vesicle returned before the ovum left the ovary, there arise several very large and exceedingly transparent vesicles: these disappear, and are succeeded by a smaller and more numerous set; several sets thus successively come into view, the vesicles of each succeeding set being smaller than the last, until a mulberry-like structure has been produced, which occupies the centre of the ovum. Each of the vesicles of which the surface of the mulberry-like structure is composed contains a pellucid nucleus; and each nucleus presents a nucleolus.

In the uterus a layer of vesicles of the same kind as those of the last and smallest set here mentioned makes its appearance on the whole of the inner surface of the membrane which now invests the yelk. The mulberry-like structure then passes from the centre of the yelk to a certain part of that layer (the vesicles of the latter coalescing with those of the former where the two sets are in contact to form a membrane), and the interior of the mulberry-like structure is now seen to be occupied by a large vesicle containing a fluid and granules. In the centre of this vesicle is a spherical body having a granulous appearance, and containing a cavity apparently filled with a colourless and pellucid fluid. This hollow spherical body seems to be the true germ. The vesicle containing it disappears, and in its place is seen an elliptical depression filled with a pellucid fluid. In the centre of this depression is the germ, still presenting the appearance of a hollow sphere. The germ separates into a central and a peripheral portion: the central portion occupies the situation of the future brain, and soon presents a pointed process which is the rudiment of the spinal cord. These parts at first appearing granulous are subsequently found to consist of vesicles.

Thus the central portion of the nervous system is not originally a fluid contained within a tube, but developes itself in a solid form before any other part. The central portion of the nervous system sometimes attains a considerable degree of development, although it be exceedingly minute; thus an instance has been met with in which the development of this part had reached a stage scarcely inferior to that in another instance, in which the corresponding part measured more than ten times the length.

There does not occur in the mammiferous ovum any such phenomenon as the "splitting" of a membrane into the so-called "serous, vascular, and mucous laminæ." Rathke had already found that parts previously supposed by Baer and others to be formed by the so-called "germinal membrane," really originate independently of it; these parts are the ribs, pelvic bones, and the muscles of the thorax and abdomen, which, according to Rathke, arise in a part proceeding out of the "primitive trace" itself. Reichert had previously discovered that the part originating the lower jaw and hyoid bone "grows out of the primitive trace." The author, beginning with an earlier period, goes farther than these observers, and shews that the so-called "primitive trace" itself does not arise in the substance of a membrane, but presents a comparatively advanced stage of the object above described as the true germ. Hence the author suggests there is no structure entitled to be denominated the "germinal membrane."

The most important of the foregoing facts respecting the development of the mammiferous ovum, however opposed they may be to received opinions, are in accordance with, and may even explain, many observations which have been made on the development of other animals as recorded in the delineations of preceding observers. If in the ovum of the bird the germinal vesicle in like manner returns to the centre of the yelk, the canal and cavity known to exist in the yelk of that ovum might be thus explained. The ovum may pass through at least one-and-twenty stages of development, and contain, besides the embryo, four membranes, one which has two laminæ, before it has itself attained the diameter of half a line, a fifth membrane having disappeared by liquefaction within the ovum.

The size of the minute ovum in the Fallopian tube and uterus affords no criterion of the degree of its development; nor do any two parts of the minute ovum, in their development, necessarily keep pace with one another.

The proportion of ova met with in these researches, which seemed to be aborted, has amounted to nearly one in eight. Sometimes two yelk-balls exist in the same ovum. With slight pressure, the ovum, originally globular, becomes elliptical. Its tendency to assume the latter form exists especially in the chorion, and seems to be in proportion to its size.

The author has discovered, that when the germinal vesicle is first seen it is closely invested by an extremely delicate membrane. This membrane subsequently expanding is that in which the yelk is formed. He has traced the chorion from stage to stage up to the period when it becomes villous, and shews that it is not, as he formerly supposed, the thick transparent membrane itself of the ovarian ovum, but a thin envelope closely investing that membrane, and not appreciable as a distinct structure until the ovum has been crushed. When the chorion first admits of demonstration as a distinct structure the ovum consists of three membranes, a state which the author has seen in an ovum no farther advanced than about an inch into the Fallopian tube. The chorion subsequently thickens and imbibes a quantity of fluid presenting a gelatinous appearance.

Notice of remarkable Agitations of the Sea at the Sandwich Islands, on the 7th November 1837. By T. Chas. Byde Rooke, F. R. C. S.

On the evening and night of the 7th November, a most remarkable commotion of the sea was witnessed at Hanolulu, in many respects similar to that witnessed at these islands in May 1819. One inch and a-half of rain had fallen during the previous twenty-four hours; the wind was fresh from the N. E., squally at intervals. The atmosphere was clear and cool. Therm. 74.5 ; the barometer had gradually fallen during the four previous days, but this evening had again risen to 30.06, at six o'clock, when the alarm was given that the sea was retiring. The first recession was the greatest, something more than eight feet; but being unprepared to make observations at the moment, the exact fall was not measured. The reefs surrounding the harbour were left dry, and the fish aground were mostly dead. The sea quickly returned, and in twenty-eight minutes reached the height of an ordinary high tide; scarcely remaining stationary, it again receded and fell six feet. This was repeated at intervals of twenty-eight minutes. On the third rising it was four inches above ordinary high-water mark, and fell again six feet four inches. After the fourth rising, the length of time occupied by the rise and fall varied, and the rise and fall diminished gradually but not regularly. At eleven P. M. the therm. stood at 74° ; barometer 30.04 ; wind freshening and frequent showers ; the ebb now occupied twenty-six minutes, and the flow ten. At eleven, 30, it became calm, with constant rain. Therm. 73.5 ; barometer, 30.03. The ebb and flow still continued occupying the same space of time, but the rise and fall decreasing. This continued during the forenoon of the 8th. The rapidity with which the water fell varied in different parts of the harbour. On the east side, the greatest rapidity noticed was six inches in a minute ; but on the north, at one time during the third recession, it fell twelve inches in thirty seconds. At no time did the water rise higher than a common spring-tide ; but the fall was about six feet below low water-mark. The same occurrence is related to have taken place in 1819, when the tide rose and fell thirteen times in the space of a few hours. On neither occasion was there any perceptible motion or trembling of the earth, or unusual appearance of the atmosphere. Since the above was written, distressing accounts have been received from Maui, and Hawaii, of the damage done to property and loss of life. On the leeward side of Maui, the same rise and fall took place as at Hanolulu ; but on the windward part of the Island, the sea retired about twenty fathoms, and quickly returned in one gigantic wave, sweeping every thing before it,—houses, trees, canoes, and every moveable object exposed to its fury. At a small village called Kahului, in the district of Walluku, on the sea retiring, the amazed inhabitants followed it as it receded, eagerly catching the stranded fish, shouting and hallooing with pleasure, when suddenly the sea rose perpendicularly before them like a precipice, and, rushing to the beach, buried the assembled multitudes in the flood, and, overflowing the shore, swept away every house in the village but one; the canoes and property of the natives were all destroyed. Happily, owing to the amphibious education of the people, but two lives were lost here; but as the same occurrence happened all along the sea-side, we shall probably hear of more deaths.

At Byron's Bay, on Hawaii, the same phenomenon took place. An unusual number of persons were collected together attending a protracted meeting, consequently every house was crowded. At half-past six, the sea retired at the rate of four or five knots an hour, reducing the soundings from five to three and a half fathoms at the anchorage, and leaving a great extent of the harbour dry. Hundreds of curious souls rushed down to witness the novelty, when a gigantic wave came roaring to the shore at the rate of six or eight knots an hour, rising twenty feet above high water mark, and fell on the beach with a noise resembling a heavy peal of thunder, burying the people in the flood, destroying houses, canoes, and fish-ponds, washing away the food and clothing of the inhabitants, large quantities of animals, firewood, and timber collected on the strand for sale. The cries of distress were horrible; those in the water, unable to swim among the wreck of houses and pieces of timber, struggling for their lives, and those on shore wailing for their friends and relatives. The British whale-ship, Admiral Cockburn, was at anchor in the bay, and to the timely aid and humane exertions of her master (Lawrence) and crew many are indebted for their lives; but for the assistance rendered by their boats, many, who were stunned and insensible, would have been carried out to sea and perished, as the natives had not a single canoe left that would float. Every thing was destroyed; those who escaped with their lives had neither food nor raiment left. In Kanokapa and Kaahelu alone sixty-six houses were destroyed, and eleven persons lost their lives, four men, two women, and five children; at Waiolama and Hauna a woman and child were drowned; at Kauwale one woman lost her life. The amount of damage done has not yet been ascertained, nor is it known how many times the sea rose and fell. There was no shock of an earthquake felt at Hilo, or elsewhere, although it is ascertained that the volcano of Kilauea was unusually disturbed the previous evening,—the fires were suddenly quenched, and yawning chasms were burst open in previously tranquil places, accompanied with violent explosions. Inquiries have been made of masters of vessels who were to the north and to the east of the islands on the 7th, at various distances, but none of them noticed any thing unusual in the sea or atmosphere. That this apparent submarine volcanic action has taken place at some distance from the islands, is proved by the wave striking the different islands simultaneously, and apparently in the same direction; but at what distance we have no means at present to determine. Perhaps the internal fires have found a new vent, which may be laying the foundation of a new group of islands in our neighbourhood. It is now 19½ years since a similar phenomenon occurred here, but not so violently as the last, nor was it attended with any loss of life. On the second day after an affecting scene was witnessed at Wailuku (Maui): the bodies that had been recovered from the sea were conveyed together to the church, followed by a great multitude: a funeral sermon was preached on the occasion;—this solemn warning made a deep, and, it is to be hoped, a lasting impression on those who witnessed it, of the uncertain tenure by which we hold our lives."—Copied from the *Ceylon Chronicle* in the *Literary Gazette* of 26th Jan. 1839.

On Photography, by ANDREW FYFE, M.D., F.R.S.E, Vice-President of the Society of Arts, Edinburgh.* Communicated by the Society of Arts.

When silver is dissolved in nitric acid, a colourless solution of lunar caustic is produced, which, when evaporated to dryness and exposed to light, becomes dark; the colour depending on the intensity of the light, and the time it has been exposed. Accordingly, paper besmeared with the solution is darkened, but if any object be put on it, so as to prevent the transmission of light, the parts covered will remain white or be tinged according to the density of the object; hence the art of *Photography*.

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* Read before the Society of Arts, on the 27th March and 10th and 17th April 1839.

It is not my intention to enter on a minute detail of this important process; my remarks will be chiefly confined to the description of improvements, which, I conceive, I have made on it since its first announcement by Mr Talbot, to whose interesting paper in the London Phil. Mag. for March of this year I must refer the reader who wishes more full information regarding it. This paper will be found also in the Athenæum and in many other periodicals.

Photography may be divided into three parts: the preparation of the paper,—taking the impressions,—and preserving them.

1. *Methods of preparing the paper.*

Though paper besmeared with solution of lunar caustic is darkened by exposure to light, it is by no means *sensitive;* other methods have therefore been recommended for preparing it for photographic purposes. That originally given by Mr Talbot is to soak it first in a weak solution of sea-salt, and, when dry, to rub it over on one side with solution of lunar caustic, by which *chloride of silver* is formed, and adheres to the paper. As thus prepared, it acquires a dark colour on exposure to light; the depth of colour depending on the strength of the solutions; hence it may vary from lilac to deep purple, approaching to black.

In preparing paper by this method, it is very difficult to get the chloride uniformly spread over its surface, and accordingly, when exposed to light, it often gives a variety of shades; indeed, in many places, it continues white. It was this that induced me to try the use of other salts of silver; and the one which I have found to answer best is the *phosphate*, procured in the usual way, by the addition of phosphate of soda to the solution of lunar caustic. In preparing the paper by this method, I generally employ one part of phosphate of soda dissolved in about eight of water, and the nitrate of silver dissolved in about six of water. The paper is first soaked in the phosphate, and then dried, after which the nitrate is put on on one side by a brush, the paper again dried and afterwards again put through the salt, by which any excess of silver is converted to phosphate. As thus prepared, it acquires a yellow tinge, which becomes black by exposure to light. It is equally *sensitive* as the chloride, and, in my opinion, gives a much more pleasing variety of shades.

Instead of preparing the paper by the process described, I frequently employ the phosphate precipitated before applying it, for which purpose the nitrate solution is dropped into that of the phosphate of soda, the yellow precipitate is allowed to fall to the bottom, and the supernatant fluid is poured off; what remains must be kept in stone bottles or in a dark place, as it is extremely sensitive to light. In preparing the paper with it, it is put on with a broad flat brush, and then dried in the usual way. Though there is a little difficulty at first in getting the phosphate uniformly spread over the surface, yet by a little practice a uniform ground is easily given, and, when once acquired, the method has the advantage of being much cheaper than those previously recommended. I sometimes add a little mucilage to the fluid, which keeps the phosphate suspended in it. There are other methods of preparing the paper, which though they do not give it so sensitive, yet are cheaper than those stated; I allude to the use of the phosphate in solution in ammonia, or, which is cheaper, in the carbonate of ammonia, which is procured by adding concentrated solution of carbonate of ammonia to the phosphate collected by precipitation as already described. A still cheaper fluid may be prepared by adding a strong solution of nitrate of silver to a concentrated solution of carbonate of ammonia, by which a *carbonate of silver* is obtained in solution, and which can be applied to the paper on one side by means of a brush. Paper thus prepared is white; it has the advantage of being easily prepared, and of giving, on exposure to light, a uniform ground which is of a brownish colour.*

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* Instead of purchasing lunar caustic of commerce, a cheaper method of ocuring it is to dissolve pure silver in nitric acid diluted with its own

2. *Methods of taking the Impressions.*

From what has been already stated, it must be evident that the most direct mode of taking the impressions is, by placing on the paper the object, the delineation of which is wished, and then exposing it to light. For this purpose it ought to be kept as close as possible on the paper, and the best method of doing so is to place it in a frame with glass in front, and a stuffed cushion behind it. The time required depends, of course, on the intensity of the light, and the density of the object; and it is of the utmost consequence to take care that it is long enough exposed, and that, at the same time, the exposure is not too long continued, for if not long enough, though the outline will be given, yet the representation will not be distinct in all its parts; whereas if too long continued, the fainter parts begin to darken, and the representation is indistinct. The time required must be found by practice. In bright sunshine one minute will be sufficient for some objects: when there is no sunshine an hour or two may be required, and in this case there is little or no danger of destroying the impression by too long exposure, as the light is not of sufficient intensity to darken too much the fainter parts.

Impressions from Engravings may likewise be got in the same way; and for this purpose, instead of using those thrown off on thin paper, by which it is supposed the light is most easily transmitted, it is, I think, better to take those on thick paper, because, though the light is not so easily transmitted, yet the impression of the engraving is much bolder, so that a more distinct delineation is given by the photographic process.

Camera Obscura.—The use of the camera obscura for photographic purposes has been described by Mr Talbot. Though representations may be got in this way, yet, so far as I have found, they have not the minute distinctness of those got by the method already noticed. Owing to the interference of the lens, the light does not act nearly so powerfully on the paper,

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bulk of water, taking care to have in the vessel more silver than the acid can dissolve; and after it has taken up as much as it can, to dilute the solution with four or five parts of water, or thereabouts, according to the colour required.

as when it has to permeate merely a frame of glass. The same is the case when the light is reflected, and hence the necessity of getting quit of the mirror placed in cameras, for throwing the representation in such a way as to allow of it being traced by the artist. Hence, in taking impressions by the camera, the prepared paper must be fixed *on the back of the box*, directly opposed to the lens, and the focus properly adjusted. I have found great advantage, in taking impressions by the camera, in using the paper moist, and keeping it so all the time it is exposed. For this purpose, after moistening it, I place it between a cushion and a pane of glass, tied tightly together, to prevent, as much as possible, the escape of moisture. In this way I have succeeded in a few minutes in getting a faint outline of the object exposed to the lens.

I may here mention that the camera affords a good method of taking profiles from busts, not by the reflected light from the bust, but by interposing it between the lens and the source of light. The bust, for instance, may be placed, during sunshine, at an open window, and the image from it thrown on the prepared paper ; using the precaution, of having the face slightly inclined towards the source of light, so as to give its outline as distinctly as possible.

Etchings.—A method of taking impressions of etchings on glass by the photographic process was described by Havell of London. For this purpose the glass is covered with etching varnish, and after the figure is etched on it, it is smoked, so as to darken the varnish to prevent the transmission of light; of course, the smoke does not adhere to those parts of the glass exposed by the etching needle, and is therefore easily wiped off with a cloth, thus leaving the etching free for the light to pass through. On exposing this with the prepared paper behind it, a beautiful impression is taken. In taking impressions in this way, the *varnished side* must be placed *next the paper*, which must be kept close upon the etching by means of a cushion, otherwise the impression is not well defined. When the glass side is next the paper, the impression is very indistinct, owing to the light, when it passes through the exposed parts of the glass, being diffused, and by which the lines run into each other.

From the ease with which impressions can be got in this way, it occurred to me that the process might be still farther extended, so as to enable us to take copies of oil paintings, or of drawings on boards, through which the light does not penetrate, and for this purpose I have followed different methods. One of these is to cover the glass with a transparent varnish, as with a thin solution of Canada balsam in oil of turpentine, and, after laying it down on the oil painting, to etch it out on the varnish, in the usual way; after this, the glass is to be slightly heated, so as to soften the varnish, which is then to be smoked, by holding it in the flame of an argand gas lamp, taking care not to soften the varnish too much; when cold, the smoke is wiped off with a cloth from the parts of the glass exposed by the etching needle. Another method is to cover one side of the glass with starch solution, of such strength, that, when dry, it is transparent, and it is then to be laid down with the glass side next the paintings, which can be traced with a pencil on the starch, and then etched on the other side, as already described. From glass etchings thus procured, impressions are taken in the usual way.

This process of transparent etching is applicable to the camera obscura; for, instead of using ground glass, as is commonly done, the representation may be thrown on starched glass, on which it is traced and then etched on the other side, as above described.

Before finishing this part of the subject, I may here allude to a method of taking the impressions, by which I have succeeded in giving them a resemblance to oil paintings.

By the method noticed, paper, or some absorbing substance, is used. I have already stated that the phosphate suspended in water may be employed, which suggested to me the use of the same substance along with a varnish, in the hopes of being able to take the impression on panel-board or metal. I have found this to answer as well as with paper. The varnish I have used is Canada balsam and turpentine, with which the phosphate, dried by the cautious application of heat, and excluded from light, is thoroughly incorporated; with this the panel-board, previously prepared as for an oil painting, is varnished; when dry, the impression is taken on it in the usual way. It will be found to have all the richness of an oil painting.

By this process, impressions equally distinct and brilliant may be taken on metal. Perhaps this may be of service in saving engravers the time and trouble of laying down on the metal the figure to be engraved.

The impressions received by the modes now described are taken by exposure to the solar ray. It is well known that the paper may be darkened by other means, as by the oxihydrogen blowpipe; but there is no necessity for having recourse to so intense an artificial light. I have found that, by concentrating the light of a common fire by metallic mirrors, the paper is darkened, and the same also occurs with the flame of a gas lamp. Of course, the time required is much longer than when exposed to sunshine. In this way I have succeeded in getting impressions of dried leaves almost as distinct as by solar light; indeed, we may dispense altogether with the mirror, for, by exposing the paper with the leaf on it, in a frame, to the light of a common fish-tail gas-burner, at the distance of a few inches, I have procured specimens some of which, though on a small scale, have all the richness of those taken by solar light.

The concentration of the rays by a metallic mirror, so as to get quit of the interference of the lens, would, no doubt, be a great improvement in the camera obscura, provided it could be accomplished. May not something of this kind be the method followed by Daguerre in getting his camera representations?

3. *Preservation of the Impressions.*

It is evident that, as the impression is produced by the agency of light on the compound of silver, when the paper is again exposed, the light will begin to act, and ultimately darken the whole, thus effacing the impression; hence the necessity of a preservative process. Two methods have been recommended by Mr Talbot, as applicable to the *chloride*, one by the iodide of potassium, the other by sea-salt. When solution of iodide of potassium is added to that of lunar caustic, a yellow iodide of silver is thrown down. The same is the case when the iodide is put on paper, previously covered with the chloride, and, provided the solution is strong, it acts also on the chloride when darkened, thus converting it to yellow iodide, which is not in the least affected by light; hence, by putting the paper with the impression through solution of the iodide, provided it is weak, the white chloride only is acted on, and being converted to iodide, is no longer liable to change. As, however, the iodide will act on the dark chloride, it is of the utmost consequence to attend to the strength of the solution, which should be such that it will not attack the faint parts of the impression. After the paper is passed through it, it should be kept for some time in water, to wash off the superfluous iodide of potassium, which, if left on, would gradually destroy the whole of the impression; indeed, even with this precaution, I find it extremely difficult to preserve them. The second method recommended by Mr Talbot is merely immersing the paper in solution of sea-salt. This process does not, however, seem to answer well; I have repeatedly failed in preserving the specimens in this way, and even when they are preserved, they are completely altered in their appearance, and deprived of their original brilliancy.

I have already stated, that I prefer the phosphate of silver for taking the impressions, not only because it is equally sensitive as the chloride, but gives a greater variety of shades. In addition to these, it has another advantage; the impressions are easily preserved. After various fruitless attempts, I at last found that the darkened phosphate is not soluble in ammonia, though, as is well known, the yellow phosphate is easily dissolved. I had, therefore, recourse to this for their preservation, and though I did not completely succeed at first, yet I at last did so, by attending to the precaution of washing off the ammoniacal solution, because, when left on, the impression gradually becomes darker and darker, and is ultimately destroyed, owing to the action of the light on it. The method I now follow is to put the paper into a diluted solution of water of ammonia (one of the spirit of hartshorn to about six of water), and leave it there till the yellow parts become white, shewing that the phosphate is dissolved, after which it is washed with water to carry off the whole of the ammoniacal solution. It should, then, when nearly dry, be subjected to pressure till dried, by which it is prevented from wrinkling, and the impression retains its original sharpness, which, unless this is done, it is apt to lose, by the fibre of the paper being raised by the repeated moistening.

Though the phosphate specimens may be preserved in this way, yet they do not retain exactly their original appearance. Those parts, whitened by the ammonia, owing to part of the silver being united with the paper, gradually acquire a faint reddish tinge,—but, though altering the appearance, it does not affect the brilliancy; indeed, in some cases, it rather improves it, by giving a pleasing tint, which contrasts well with the darker parts, and gives the appearance of colouring. I have also found that carbonate of ammonia answers equally well, and, being much cheaper, it will of course be preferred. I generally employ a solution, prepared by dissolving one part of salt in about four of water, in which the paper is kept for a minute or so, and then afterwards washed, and subjected to pressure, as already noticed. Impressions thus preserved acquire the same reddish tinge as those acted on by ammonia.

I have before stated that the paper may be prepared by washing it over with a solution, procured by adding nitrate of silver to carbonate of ammonia. The impressions taken with that paper are easily preserved, by merely washing them with water, to carry off the part not acted on by the light, which is another advantage, in addition to those stated, for using the carbonate solution. Like the phosphate specimens, they also acquire a reddish tint.

Other preservative methods have been recommended, as, by covering the impressions with a yellow colour, to prevent, as much as possible, the transmission of the chemical ray of the light; but those above stated, particularly when the phosphate or carbonate is used, are so simple and efficacious that it is unnecessary to allude to them.

Before finishing this part of the subject, I may here allude to a valuable practical application of photography, in diminishing the labours of the lithographer. In communicating the impression of any object to the stone, as of a dried plant, or in copying an engraving, it is necessary to trace them on paper, and, after again tracing them with the transfer ink, to transfer them to the stone. Now, by receiving the impression on paper by the photographic process, all the labour of the first tracing is avoided. But there is no necessity for using paper, as the impression may at once be communicated to the stone, which easily receives the phosphate, and which may therefore be prepared in the same way as the papers, and the impression also taken in the usual manner, after which it is traced over with the transfer ink. By this process not only is a great deal of labour saved, but the representation must be much more exact than when traced; for though by the latter the outline is correct, yet much is left to be afterwards filled in by the eye, whereas, by the photographic process, every, even the most minute filament, is distinctly and accurately laid down on the stone.*

Method of taking Impressions in which the lights and shades are not reversed.

By the different methods now described for getting photographic impressions, the lights and shades are always reversed, because, as it is by the action of the light that the compound of silver is darkened, wherever it is prevented from penetrating, the paper retains its original colour. Though the impressions thus procured are accurate as to outlines, yet, in many cases the representation is far from being pleasing; it is therefore a great desideratum to have a method of getting impressions in which there is no reverse; in fact, to give a true representation of the object, and in this I have succeeded by the use of the iodide of potassium. I have already stated, that, when the darkened phosphate is exposed to the iodide, it is instantly converted to yellow, provided the solution is of sufficient strength; if weak, the action goes on slowly. In some impressions which I had attempted to preserve in this way, I observed, that, when exposed to light, they began to fade, which induced me to try the effect of light on darkened paper, soaked in solution of iodide, of such strength, that it just failed to attack it instantly. In my first attempt I succeeded in bleaching the paper, but in my next I failed. On considering the circumstances under which these trials were made, I found that the only difference between them was, that in the first the paper was moist, in the last it was dry. Accordingly, on repeating the experiment with the paper moist, I again succeeded in getting a delineation of the object placed on the paper, as distinct and altogether as brilliant as those obtained by the other process.

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* For this method of applying the photographic process I am indebted to Mr Nichol, lithographer, by whom lithographic impressions, thus taken, were exhibited to the Society of Arts. As a proof of the value of this process, I may also mention, that, on the evening of the 17th of April, when I exhibited a photographic specimen of dried ferns, it was, by Mr Forrester, lithographed, and impressions taken from it in the course of two hours; had this been done in the usual way, it would have required many hours of labour, and, after all, not have given such accurate delineations.

The method which I now follow is, after preparing the phosphate paper, to darken it, then immerse it in solution of iodide of potassium, of such strength that it does not act instantaneously, and, *when still moist*, to expose it to light with the object on it, and continue the exposure till the exposed part of the paper becomes yellow. In this case, there is a tendency in the iodide to convert the dark phosphate to yellow iodide, which would go on slowly, but is hastened by the light; of course, if the object on the paper is impervious to light, the impression is black throughout, but if it is of different density, so as to allow the light to be differently transmitted, the impression presents the lights and shades as in the object itself; because those places behind the dense pieces retain their original blackness, while those behind the less dense are more or less bleached, just according to the transmission of the light. When impressions thus procured are kept, they begin to fade, owing to the slow but continued action of the iodide of potassium; hence the necessity of a preservative process. After repeated trials, I have found that by far the simplest and the best is merely immersion in water, so as to carry off the whole of the iodide of potassium not acted on by the phosphate, and by which any farther action is completely prevented. By this method, the specimens do not lose in the least their original beauty, and they may be exposed to continued sunshine without undergoing the slightest alteration.

I have succeeded also in taking impressions with the chloride in the same way—but it is necessary, for the success of the process, to use the solution of the iodide much weaker than for the phosphate, because the chloride is more easily acted on. In both cases it ought to be made of such strength that it just acts, and then, before using it, it must be weakened by the addition of a little water. For the phosphate, it will be found, in general, that 1 of salt to 10 of water, and for the chloride, that about 30 of water, will give a solution of the requisite strength. Of course, in preserving the specimens, the precautions as to washing and pressure must be attended to.

Notes on Daguerre's Photography. By Sir JOHN ROBISON, Secretary to Royal Society of Edinburgh, &c. &c. (Communicated by the Society of Arts).

Sir—In compliance with the request that I should commit to writing and put into your hands the substance of what I communicated to the Society of Arts in reply to the questions put to me at the last meeting, I beg to state, that circumstances having led to my being included in a small party of English gentlemen who were lately invited to visit the studio of M. Daguerre, to see the results of his discovery, I had an opportunity of satisfying myself, that the pictures produced by his process have no resemblance to any thing which, as far as I know, has yet been produced in this country; and that, excepting in the absence of colour, they are as perfect images of the objects they represent, as are those which are seen by reflection from a highly polished surface. The perfection and fidelity of the pictures are such, that, on examining them by microscopic power, details are discovered which are not perceivable to the naked eye in the original objects, but which, when searched for there by the aid of optical instruments, are found in perfect accordance: a crack in plaster, a withered leaf lying on a projecting cornice, or an accumulation of dust in a hollow moulding of a distant building, when they exist in the original, are faithfully copied in these wonderful pictures.

The subjects of most of the numerous specimens which I saw, were views of streets, boulevards, and buildings, with a considerable number of what may be termed interiors with still life; among the latter were various groups made up of plaster-casts and other works of art. It is difficult to express intelligibly a reason for the charm which is felt in beholding these pictures; but I think it must arise, in some measure, from finding that so much of the effect which we attribute to colour, is preserved in the picture, although it consist only in light and shade; these, however, are given with such accuracy, that, in consequence of different materials reflecting light differently, it is easy to recognise those of which the different objects in the groups are formed. A work in white marble is at once distinguished from one in plaster-of-Paris by the translucency of the edges of the one, and the opacity of the other. Among the views of buildings, the following were remarkable: A set of three pictures of the same group of houses, one taken soon after sunrise, one at noon, and one in the evening; in these the change of aspect produced by the variations in the distribution of the light, was exemplified in a way which art could never attain to.

One specimen was remarkable from its shewing the progress made by light in producing the picture. A plate having been exposed during 30 seconds to the action of the light and then removed, the appearance of the view was that of the earliest dawn of day; there was a grey sky, and a few corners of buildings and other objects beginning to be visible through the deep black in which all the rest of the picture was involved.

The absence of figures from the streets, and the perfect way in which the stones of the causeway and the foot-pavements are rendered, is, at first sight, rather puzzling, though a little reflection satisfies one that passing objects do not remain long enough to make any perceptible impression, and that (interfering only for a moment with the light reflected from the road), they do not prevent a nearly accurate picture of it being produced.

Vacillating objects make indistinct pictures, *e.g.* a person getting his boot cleaned by a decrotteur gave a good picture, except that having moved his head in speaking to the shoeblack, his hat was out of shape, and the decrotteur's right arm and brush were represented by a half-tinted blot, through which the foot of the gentleman was partially visible.

There can be no doubt that, when M. Daguerre's process is known to the public, it will be immediately applied to numberless useful purposes, as, by means of it, accurate views of architecture, machinery, &c., may be taken, which, being transferred to copper or to stone, may be disseminated at a cheap rate; and useful books on many subjects may be got up with copious illustrations, which are now too costly to be attainable: even the fine arts will gain, for the eyes accustomed to the accuracy of Daguerrotype pictures, will no longer be satisfied with bad drawing, however splendidly it may be coloured. In one department, it will give valuable facility. Anatomical and surgical drawings, so difficult to make with the fidelity which it is desirable they should possess, will then be easily produced by a little skill and practice in the disposition of the subjects and of the lights.

It is a curious circumstance that, at the same time that M. Daguerre has made this beautiful and useful discovery in the art of delineation, another Parisian artist* has discovered a process by which he makes solid casts in plaster of small animals or other objects, without seams or repairs, and without destroying the model, (Moulage d'une seule pièce, sans couture ni reparage, et avec conservation parfaite du modele). I am in possession of several specimens of his work, among which are casts of the hand of an infant of six months, so delicately executed, that the skin shews evident marks of being affected by some slight eruptive disease. I am, dear Sir, very faithfully yours,

Edinburgh, 1*st June* 1839. JOHN ROBISON.

JAMES TOD, Esq. Secretary to the Society of Arts.

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* Hippolyte Vincent, Mouleur, Rue Neuve St Francois No. 14 (au Marai).

Note by Dr Daubeny *to his Memoir, contained in the Edinburgh New Philosophical Journal for April* 1839, *in Reply to Professor Bischof's Remarks on the Theory of Volcanos.* (Communicated by the Author.)

I hasten to correct an error, which a friend has had the goodness to point out to me, in the calculation entered into in my reply to Professor Bischof, concerning the relative weight of the materials of which ordinary lava consists, compared to that of the metallic bases which are included in its composition.

In this calculation I had erroneously estimated the specific gravity, both of the lava, and of its metallic constituents, from the proportion of their respective ingredients by *weight*, and not, as ought to have been done, from that of their respective *bulks ;* thus giving to the lava itself a specific gravity of 3.206, and to its metallic constituents one of 3.40 ; whereas the true specific gravity of the former turns out to be 3.09, and that of the latter only 2.39.

Although I felt the propriety of correcting this inadvertence from the moment it had been pointed out to me by my intelligent correspondent, I do not conceive that my argument is materially weakened by its admission.

Whilst the chemical theory of volcanos supposes those portions of the interior of the globe in which volcanic operations take place, to contain, among other constituents, the metallic bases of the substances which are found to constitute lava, the opposite hypothesis necessarily assumes, that the materials of the lava themselves existed before their ejection in the same position.

But, on either supposition, we have equally to encounter the difficulty arising from the high specific gravity which mathematicians have assigned to the internal contents of our globe, and consequently are driven to the alternative of imagining, either that some denser material exists at a still greater depth than that from which the lava issues, or that the lava itself has had its density proportionally increased by the superincumbent pressure.

Now, under such circumstances, the difficulty would be not materially enhanced, if the specific gravity of the ejected material had chanced to be only 2.39, instead of being, as it now turns out, 3.09, and this, as we have seen, is the whole amount of the difference, between the density of the lava itself, and that of the metals which are present in its composition.

I find, also, that, by my haste to reply to Bischof's objections, I have been prevented from noticing certain passages which appeared, since my remarks were penned, in the concluding portion of the Professor's memoir, having reference to my own views and arguments. I am not, however, disposed at the present moment to enter further at length into this discussion, and will only remark, that Dr Bischof is correct in concluding, that the chemical processes which I assign as the cause of the temperature of thermal springs, are not imagined to be going on near the surface of the globe.

If there be reason for concluding that volcanic phenomena result from certain chemical processes, the latter must at once lie very deep, and must take place throughout the globe very generally, although in various degrees of intensity.

It does not, therefore, follow from this, that volcanos, which constitute only the most violent phase of activity of which these operations are susceptible, should be themselves universally distributed; but it does appear a natural consequence that thermal springs, which are the results of a more languid state of action of the same kind, should be as generally diffused, as we find them to be. Not, indeed, that the processes alluded to are necessarily going on everywhere, in the very spots from which thermal springs issue, but that a general elevation of temperature has been imparted to the materials of our globe at a certain depth beneath its surface, owing to those operations of a volcanic nature which are here and there in progress.

With regard to the precise nature of the chemical processes which give birth to volcanos, I have always spoken with caution, studiously distinguishing between the degree of probability belonging to that part of the hypothesis in question which asserts the fact of the general absorption of oxygen gas throughout the interior of the globe, and that which undertakes to explain the latter phenomenon, by supposing certain bodies, capable at once of decomposing water, and of combining with its oxygen without eliminating a volatile product, to exist in the spots at which the action originates.

Whatever difficulty there may be in imagining the earthy and alkaline metalloids to exist amongst the number of these bodies, that difficulty will at least not be enhanced by supposing them to be as generally distributed as the phenomena themselves appear to warrant us in supposing. If it be once granted, that potassium and sodium contribute, by their combustion, to feed the fires that glow beneath Vesuvius or Etna, there seems no reason why we should refuse to believe that the same substances may be present, wherever thermal waters or earthquakes indicate a similar train of phenomena.

With these few remarks I am content to close the discussion, and shall not be tempted to resume it, until it shall either be shewn that the chemical phenomena observed during the several phases of volcanic action are inconsistent with the conditions of the theory I have adopted, or else that they can be satisfactorily deduced from the supposed existence of a high temperature in the interior of the globe.

OXFORD, *June* 4. 1839.

On the Mineralogical Nature of Terrestrial, Fluviatile, and Marine Shells. By M. L. A. NECKER.

BREWSTER has remarked that mother-of-pearl, like arragonite, possesses two axes of double refraction, (*Bibliothèque Universelle de Genève*, vol. ii. p. 182, March 1836.) Subsequent observations, by shewing that several species of terrestrial and aquatic shells are otherwise connected with arragonite, prove that this substance, and not calcareous spar, is the matter of which almost all shells are composed.

On examining, with the magnifying glass, a *Limacella*, that is to say, the interior shell of a grey and black Limax,[*] I found that the large mass of translucent, colourless, calcareous matter, which is covered by a surface having the form of a shell, afforded distinct traces of crystalline facets, some of which appeared to be triangular like those of the diedral terminations of arragonite, and others in the form of elongated parallelopipeds like the faces of the prism of the same mineral. I could not reduce these faces to the rhombohedral system of the genus calcareous spar; and thus, although I could not determine the form of these crystals, owing to their being so confusedly grouped together, this circumstance, in addition to the complete absence of lamellar texture, a slightly resinous though pretty high lustre, and an appearance perfectly analogous to that of arragonite, appeared to me entirely to distinguish this crystalline mass from calcareous spar. Besides, it makes a deep scratch on the clear and crystallized Iceland-spar.

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[*] Limax maximus.

I afterwards remarked that the shells of the *Helix pomatia*, the *Anadonta anatina*, and the *Unio pictorum*, likewise scratched the Iceland-spar. All of them, as well as the *Limacella*, effervesce briskly with nitric acid. The *Anadonta* has two layers of nearly equal thickness, the upper composed of crystalline prisms with axes parallel to each other and perpendicular to the plane of the layer, the lower of compact mother of pearl. In the *Unio pictorum*, the upper layer is very thin, and the mother of pearl very thick.

I now give the list of all the shells which I have tried, and which scratch the crystallized calcareous spar more or less strongly.

Terrestrial and Fluviatile Shells.

Limacella, (strongly). *Helix pomatia*, (rather strongly). *Helix nemoralis*, yellow, adult, having a perfect mouth, (strongly). *Helix nemoralis*, yellow, young, with an imperfect mouth, (feebly). *Helix carthusianella*, living shell, with the mouth, (very feebly on account of its slight thickness and its great fragility). *Helix ericetorum*, (rather strongly). *Physa fontinalis*, becomes abraded at the thinnest side of the mouth, but it scratches deeply, although, on account of its fragility, much pressure cannot be employed. *Lymnæus auricularis*, (scratches, although it is fragile). *Lymnæus stagnalis*, (scratches strongly, although fragile). *Anadonta anatina* (strongly). *Anadonta cygnæa*, (rather strongly). *Unio pictorum*, (strongly). *Cyclas rivalis*, dead and altered, (scratches deeply, but becomes itself abraded).

Marine Shells.

Ostrea edulis, (scratches very strongly). *Ostrea parasitica*, (still more so), *Anomia ephippium*, (feebly). *Anomia cylindrica*, (very feebly, on account of its great fragility). *Mytilus edulis*, (strongly). *Lutraria elegans*, Fleming, (strongly). *Mya truncata*, (strongly). *Mactra stultorum*, (strongly, although fragile). *Cardium aculeatum*, (strongly). *Cyprina islandica*, (strongly). *Venerupis perforans*, (strongly). *Pecten opercularis*, (more or less strongly). *Solen siliqua*, (not strongly, although thick). *Solen ensis*, (strongly, although fragile). *Balanus* (?) (strongly). *Pholas crispata*, (strongly).

It is remarkable, that two genera of perforating shells, the *Pholas* and the *Venerupis*, scratch calcareous spar strongly. Thus, the rugosities with which their shells are provided may assist, along with the acids with which they are furnished, to excavate the calcareous rocks which they inhabit. The idea that these shells were composed of carbonate of lime, seemed to render it impossible that they should perforate calcareous rocks, whose hardness was supposed to be equal to their own. It is now evident, that, as they are composed of arragonite, they may act mechanically even on the hardest limestones.

If we add to this hardness a specific gravity also higher than that of calcareous spar, as found by M. de la Beche, there can be no doubt that the substance of most of these shells is arragonite. In fact, this specific gravity is, in the shells determined by M. de la Beche, in most instances higher than 2.7, which is that of calcareous spar, and is even as high as 2.8 in one case. The specific gravity of arragonite is 2.9; but we must recollect, that in shells the calcareous mineral matter is always mixed with organic matter, whose density must be very inconsiderable, and thus proportionably diminish the specific gravity.

Perhaps this last-mentioned matter exists in a greater proportion in those shells cited by M. de la Beche, whose specific gravity is below 2.7. Perhaps, also, calcareous spar may, to a certain extent, enter into the composition of certain species of shells; and this would explain how a large *Strombus*, afforded to the Count de Bournon, in an accidental fracture, the incidences of the faces of the primitive rhombohedron of calcareous spar. The two layers, of which certain shells are composed, as the *Anadontas* and the *Unios*, may be, the one calcareous spar, and the other arragonite. If, then, the Strombus, cited by the Count de Bournon in his work on carbonate of lime and arragonite, should be of the same nature as the shells last mentioned, it must be believed that it is the layer of the spar which caused a fracture in the rhombic form in the individual in question.—(*Annales des Sciences Naturelles*, 1889.)

On an interesting mode of occurrence of Calcareous Spar in Basalt-tufa. By William Haidinger, Esq. F.R.S.E. and Member of the Wernerian Natural History Society.

Fresh sections have lately been exposed, during the excavations carried on near Schlackenwerth, with the view to obtain water for the new furnaces of Prince Metternich. In these I lately had an opportunity of observing a mode of occurrence of calcareous spar, which, on account of the conclusions to be deduced from it, is particularly calculated to attract the attention of those who investigate the changes which the crust of our globe has undergone. I have to thank the kind orders of the Imperial Secretary Dr A. Schmidt, for an extremely interesting series of the specimens, and have also to acknowledge the attention of Mr Kellerman, who forwarded them to me.

Between the layers of a more or less compact basalt-tufa, there occur masses, from whose form and surface it seems evident that they were originally trunks of trees. The direction in which they lie is from west to east. They are of various diameters, being generally from one to eight inches thick. The most remarkable circumstance connected with them is their internal structure, which is displayed when the stems are broken asunder. As they lie between the layers, the interior, which was previously filled by the wood, exhibits the phenomenon represented in the wood-cuts, and in which Fig. 1 is a transverse, and Fig. 2 a longitudinal section. It is there seen that the interior is replaced by radiated groups of crystals, which, proceeding from centres *a a*, and chiefly from the upper side, extend to the opposite walls. The small quantity of organic matter left behind is deposited in the lower portions *b b* in parallel fibres.

Fig. 1.

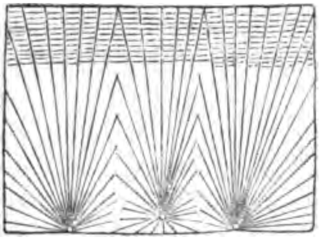
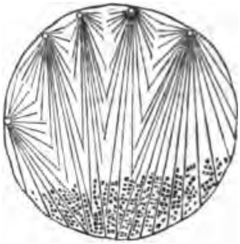


Fig. 2.



According to the form, the radiated portions and the crystals must have been originally arragonite or prismatic lime-haloid. When, however, these are broken in two, there is no trace of the crystalline structure and conchoidal transverse fracture of that species, but, on the contrary, we find a combination of individuals of the rhombohedral lime-haloid or calcareous spar. The previously formed crystals of arragonite, therefore, have been converted into calcareous spar by a subsequent supplementary process. In conformity to the experiments of Gustav Rose, we may assume that the pseudomorphosis of arragonite in the wood has taken place during a high temperature, while that of calcareous spar in arragonite has occurred at a low temperature. The deposition of the basaltic tufa on the east side of the basaltic outburst, the flowing in of the boiled wood between the layers, occurred during a high temperature, while the waters found an outlet to the east, at the same period of time in which, by means of the elevation of the land to its present height, the valleys of the Elbe and Eger throughout their whole extent, burst across the solid land. The formation of the crystals of arragonite occurred during the first portion of the period of cooling; and the conversion of the arragonite into calcareous spar during the second portion, during which circumstances more nearly approached the state of matters at the present day, and which is perhaps not yet arrived at a termination. As in other places in the neighbourhood, for example at Waltsch, arragonite is contained in a similar rock, without being converted into calcareous spar; further observations are necessary to decide, if rapid evaporation on one side and moist pressure on the other were the circumstances which produced this difference.

Elbogen, 8th *June* 1838.

His Excellency Count K. Sternberg added the following information on the subject to the above very interesting notice by Mr Haidinger.

On the right bank of the stream which flows through Schlackenwerth, there is a range of hills running from south to north, which is covered by wood, and has a continuation to the east; at their base a canal is being formed, and near it a road, which is to lead to a new iron-work. In order to obtain the necessary space for this purpose, and to prevent the descent of the weathered rock, the southern declivity has been dug away to the extent of 4 fathoms in height and 2 fathoms in breadth. For a distance of 150 paces from the western point of these operations, there are found in a basalt-tufa, having its surface composed of knolls and turret-shaped masses, a vast number of stems of trees from 2 to 7 inches in diameter, partly perpendicular, partly oblique, and partly horizontal, all of which have their interior filled with calcareous spar. There were also found close at hand similar round spaces, in which stems of trees had existed, but which had rotted out and had not been replaced. Owing to their being filled with water, we cannot yet ascertain what still remains of woody fibres in these cavities, which can be sounded to a depth of 2 or 3 yards. We can follow those lying transversely to a distance of two or three fathoms. That stems of trees actually existed here, is proved by separate fragments, in which we can recognise the woody fibres, but no further organization. Near these stems, in basalt-tufa, and deeper, in a flat layer of the rock, there are impressions of leaves with a middle rib, and many secondary veins, consequently of dicotyledonous plants. It is thus evident that a wood stood here which was enveloped in the liquid basalt. It would almost seem that the greater part of the plants were gradually decomposed, and that the liquid from which the calcareous spar and arragonite were precipitated, filled up the cavities produced by the decomposition; for it is remarkable how the radiated portions of calcareous spar, which, on a transverse fracture, proceed from more than one point, never cross one another, and also terminate in the round form of the tree. I recollect no other example of an analogous phenomenon.

In the eastern continuation of the hills basalt occurs, but no stems of trees have been met with.—*From Poggendorff's Annalen*, 1838, vol. xlv. p. 179.

A new method of Reshipping a Rudder at Sea, and that with ease in the strongest weather. By MARTIN J. ROBERTS, Corresp. Mem. R. G. S. of Cornwall, M. L. E. S., &c. Communicated by the Society of Arts.*

The late gales, or rather hurricanes, have claimed the attention of scientific men as phenomena worthy of elucidation, and we must rejoice that the task of collecting and arranging the facts for this purpose has fallen on so able a man as Mr Scott Russell.

But these gales have also called our attention in another and more painful manner, and our sympathies have been awakened by accounts of the dreadful loss of life and property that has taken place on our own shores. I may well quote an old song, and say

> "Ye gentlemen of England, who live at home at ease,
> How little do you think upon the dangers of the seas."

Amongst these dangers, a frequent and a most fearful accident is, the loss of the ship's rudder; when this occurs, she lies a helpless log at the mercy of the waves, liable to broach to, and be thrown on her beam ends, or have every thing washed off her deck. This accident (the loss of the rudder) lately befel the Leith packet, and had she been a sailing vessel instead of being a steamer, she would not have escaped so easily.

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* Report of the Committee appointed to inquire into Mr Roberts's new method of Reshipping a Rudder at sea.

Committee—Admiral Sir D. Milne, Rd. Hunter, Esq., D. Stevenson, Esq., J. Slight, Esq.

Present—Sir David Milne, Rd. Hunter, Esq.

The Committee consider the invention to be valuable, and calculated to afford, in many cases of extreme danger, a speedy means of repairing the damage.

The chafing on the grommet must be very great, and possibly rope made of American hides might be found to withstand it, if the wire rope should not prove sufficiently durable. The Committee think that many ships might be saved if the precautions recommended by Mr Roberts were universally adopted. RD. HUNTER, *Convener.*

April 26. 1839.

The loss of the rudder is an accident of which every seaman has a great dread, and if any method can be pointed out whereby he can in a few minutes remedy this misfortune, he will, I am sure, hail it as a great boon.

As I need not enlarge upon the importance of the object, I beg leave, without further preamble, to offer to this Society's, and to the seamen's notice, a method of reshipping a rudder at sea, and this with ease in the heaviest gale of wind.

The apparatus necessary for this purpose is simple, and is also cheap, an advantage that will have great weight with shipowners. The system of insurance is one that leads to great carelessness of the means of preserving life and property at sea; and any plan for assisting in such preservation, will (I am sorry to say) meet with but little countenance, unless it is cheap, and gives but little trouble.

As an assistance in explaining my invention, I have brought a model of the stern post and rudder of a ship, shewing my method of reshipping a rudder.

Before the ship leaves her dock, a hole must be bored in the heel of the stern post, of sufficient size to allow of two small ropes being rove through it; these ropes I would recommend should be of wire, such as I have used in my ship's lightning conductors; they are made of copper wires laid up as a common hemp rope, are very flexible, can be rove through a small sheave, and possess great strength in a small compass. Let two ropes be rove through the hole in the heel of the stern post, and both parts of each rope be brought in board through the rudder case; being of a small size, they will offer but little obstruction to the ship's progress, or to prevent them from doing so in the slightest degree, a groove may be made in the sides of the stern post for the ropes to lie in. These ropes must, of course, be rove before the ship leaves the harbour, and at sea they must be overhauled, or worked backwards and forwards occasionally (say once a-week) to keep all clear for running, should an accident to the rudder require their use.

Let us now suppose the ship's rudder to be carried away. In general, the rudder is only unshipped, and can be recovered by means of the rudder chains; when this is the case bring the rudder upon deck. But if the rudder is totally lost, make use of a spare one, which can be easily carried in separate pieces; or in default of this, rig up a rudder of spars and ropes on Captain Pakenham's, or on any other easier plan. Having your rudder upon deck, let a hole be bored through the heel, and in such a part as will correspond with the hole in the stern post when the rudder is in its proper position. Through the hole in the rudder work a grommet, either of rope or wire, but very strong, for this grommet must traverse freely in the hole. Now bring in over the quarter an end of each rope, rove through the stern post, and make both fast to the grommet in the heel of the rudder. Then drop a guy rope through the rudder case, and bring the end in over the quarter, and make it fast to the head of the rudder. All is now prepared. Heave your rudder overboard, haul upon the guy made fast to the rudder-head (this will lead it to the rudder-case), and rouse in the slack of the rudder heel-ropes. Bring the rudder-head up the rudder-case, and, when high enough, haul taut and belay your heel-ropes. The rudder-head guy-rope, which should be a strong one, may be now made fast to a spar going across the deck, or a frame-work may be made above the rudder-head, to which the head-rope may be fixed; this is to support the weight of the rudder, and thus take off the strain from the heel-ropes. But in a two-decked ship, as in a ship of war, the head rope may be fixed to a carline between the beams of the deck, immediately above the rudder-head. Every thing is now in its place, and the rudder is quite as secure, if not more so, than when the ship left her port.

Those gentlemen in this Society who are seamen will immediately perceive the advantages of my plan; but to those who are not, I may as well state that the great objection to all plans before suggested for reshipping a rudder is the difficulty of keeping the heel of the rudder down; this arises from several causes, viz., its own buoyancy, the ship's forward motion, and the action of the waves. All these causes combine to throw up the heel of the rudder, render it useless in steering the ship, and tear away the cumbrous apparatus of guys, &c. usually led under the bottom of the ship with the intention of keeping the heel of the rudder down. But by the method I have now suggested, the rudder will be maintained in a good position, and in perfect security.

My plan is, as I have before said, cheap and simple, and it will give me heartfelt satisfaction if this suggestion is ever found useful in a time of need.

MARTYN J. ROBERTS.

Notice of a cheap and simple method of preparing paper for Photographic Drawing, in which the use of any salt of silver is dispensed with. By MUNGO PONTON, Esq., F.R.S.E., Foreign Secretary Society of Arts for Scotland. Communicated by the Society of Arts.*

While attempting to prepare paper with the chromate of silver, for which purpose I used first the chromate of potash, and then the bichromate of that alkali; I discovered that when paper was immersed in the bichromate of potash alone, it was powerfully and rapidly acted on by the sun's rays. It accordingly occurred to me, to try paper so prepared to obtain drawings, though I did not at first see how they were to be fixed. The result exceeded my expectations. When an object is laid in the usual way on this paper, the portion exposed to the light speedily becomes tawny, passing more or less into a deep orange, according to the strength of the solution, and the intensity of the light. The portion covered by the object retains the original bright yellow tint, which it had before exposure, and the object is thus represented yellow upon an orange ground, there being several gradations of shade, or tint, according to the greater or less degree of transparency in the different parts of the object.

In this state, of course, the drawing though very beautiful, is evanescent. To fix it, all that is required is careful immersion in water, when it will be found that those portions of the salt which have not been acted on by the light are readily dissolved out, while those which have been exposed to the light are completely fixed in the paper. By this second process, the object is obtained white upon an orange ground, and quite permanent. If exposed for many hours together to strong sunshine, the colour of the ground is apt to lose in depth, but not more so than most other colouring matters.

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* Read before the Society of Arts for Scotland 29th May 1839.

This action of light on the bichromate of potash differs from that upon the salts of silver. Those of the latter which are blackened by light, are of themselves insoluble in water, and it is difficult to impregnate paper with them in an equable manner. The blackening seems to be caused by the formation of oxide of silver. In the case of the bichromate of potash, again, that salt is exceedingly soluble, and paper can be easily saturated with it. The agency of light not only changes its colour, but deprives it of solubility, thus rendering it fixed in the paper. This action appears to me to consist in the disengagement of free chromic acid, which is of a deep red colour, and which seems to combine with the paper. This is rendered more probable from the circumstance that the neutral chromate exhibits no similar change.

The active power of the light in this instance, resides principally in the violet rays, as is the case with the blackening of the salts of silver. To demonstrate this, three similar flat bottles were filled, one with ammoniuret of copper which transmits the violet rays, one with bichromate of potassa transmitting the yellow rays, the third with tincture of iodine transmitting the red rays. The paper was readily acted on through the first, but scarcely if at all through the second and third; although much more light passed through the bottle filled with bichromate of potassa than through the one filled with ammoniuret of copper.

The best mode of preparing paper with bichromate of potash is to use a saturated solution of that salt; soak the paper well in it, and then dry it rapidly at a brisk fire, excluding it from day light. Paper thus prepared acquires a deep orange tint on exposure to the sun. If the solution be less strong or the drying less rapid the colour will not be so deep.

A pleasing variety may be made by using sulphate of indigo along with the bichromate of potash, the colour of the object and of the paper being then of different shades of green. In this way also the object may be represented of a darker shade than the ground.

Paper prepared with bichromate of potash is equally sensitive with most of the papers prepared with salts of silver, though inferior to some of them. It is not sufficiently sensitive for the camera obscura, but answers quite well for taking drawings from dried plants, or for copying prints, &c. Its great recommendation is its cheapness and the facility with which it can be prepared. The price of the bichromate of potash is 2s. 6d. per lb., whereas of the nitrate of silver only half an ounce can be obtained for that sum. The preparing of paper with the salts of silver is a work of extreme nicety, whereas both the preparing of the paper with the bichromate of potash and the subsequent fixing of the images are matters of great simplicity, and I am therefore hopeful that this method may be found of considerable practical utility in aiding the operations of the lithographer.

M. PONTON.

Edinburgh, 18*th May* 1839.

Address to the Geological Society of London, delivered at the Anniversary, on the 15*th of February* 1839. By the Rev. WILLIAM WHEWELL, B.D., F.R.S., President of the Society.*

" GENTLEMEN,—In attempting a sketch of the subjects which have occupied the attention of the Society during the year, I should wish to retain that distribution of the science of geology according to which I arranged my remarks in the Address which I had last year the honour of reading to the Society ; I mean the primary division into *Descriptive Geology* and *Geological Dynamics;* the former implying a description of the rocks of the earth's surface according to an established classification of strata and formations; and the latter dealing with the study of those general laws and causes of change by which we hope to understand and account for the facts which Descriptive Geology brings before us;—in short, the present condition and the past history of the earth's crust. But as the laws of permanence and change, with regard to organized beings, differ very widely from the dynamics of brute matter, we may conveniently make a separate study of the relations of organic life to which geology conducts us, and may mark it by the name *Palæontology*, by which it is commonly known. I will add that it still appears to me convenient, for the present, to divide Descriptive Geology into two portions,—the Home circuit, in which the order of superposition has already been established with great continuity and detail; and the Foreign region, in which we are only just beginning to trace such an order. I shall also, as before, take the ascending order of strata. According to this arrangement of the science, I shall venture to bring to your recollection a few of the points to which our attention has mainly been called during the past year.

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* The biographical part of this Address, want of room forces us to delay till another opportunity.

Descriptive Geology.

" 1. *Home* (*North European*) *Geology.*—When I stated that Descriptive Geology has for its task the reference of the rocks of some portion of the earth's surface to an established classification into strata and formations, it was implied, that the more common employment of the descriptive geologist must be to refer the rocks which he examines to some classes *already fixed and recognised;* but it could hardly fail to occur to you, that from time to time the leaders in this study will be called upon to execute a more weighty and elevated office, in framing the classifications which other observers are to apply; in drawing the great lines of division and subdivision which fix the form of the subject; in setting up the type with which examples are to be compared; in constructing the language in which others are to narrate their facts. Steps of this kind have formed, and must form, the great epochs in the progress of all science of classification, and especially in ours; and I need not remind you how great the importance and the influence of such steps amongst you have been. To pronounce at once upon the success of such steps must always be in some degree hazardous; since their success is in fact this, that they influence permanently and powerfully the researches, descriptions, and speculations of future writers; and there are few of us who can pretend to the foresight which might enable us to say, in any special case, how far this will be so. Yet the great works of Messrs Murchison and Sedgwick, tending to the establishment of a classification of the strata below the old red sandstone (works which, on all accounts, we must consider as a joint undertaking), appear already to offer an augury which can hardly be doubtful, of this influence and permanence. Mr Murchison's appellation of the "Silurian System" has already been adopted by MM. Elie de Beaumont and Dufresnoy, who have given it currency on the Continent: M. Boué and M. de Verneuil announce the diffusion of "Silurian" rocks in Servia and the adjacent parts of Turkey in Europe; our own members, Mr Hamilton and Mr Strickland, have extended their range to the Thracian Bosphorus; M. Forchhammer of Copenhagen, visited the "Silurian region" to recognise the rocks of Scandinavia; and MM. Omalius D'Halloy and Dumont have just explored it, to establish a parallel between its deposits and those of Belgium. It will be observed that some of the districts thus mentioned are out of the limits of our geological Home circuit; and if the identification be really and permanently established in these cases, will extend the limits within which the parallelism of geological series can be asserted: and this is, in effect, what we have a right to look for, sooner or later, in the progress of geological science. As we must be careful not to apply our domestic types without modification to other regions, so must we take care not to despair of modifying our scheme, so that it shall be far more extensively applicable than it at first appeared to be. Of this progress of things, examples are too obvious and too recent to require to be pointed out.

The labours of Professor Sedgwick refer to the "Cambrian System," which lies beneath the Silurian system, occupying much of North Wales, Cumberland, and a great part of Scotland; while the Silurian system spreads over a great part of South Wales and the adjoining English counties. The classification of the rocks of this portion of our island to which Professor Sedgwick has been led, though laid before you only at a recent meeting, is the fruit of the vigorous and obstinate struggles of many years, to mould into system a portion of geology which appeared almost too refractory for the philosopher's hands; and which Professor Sedgwick grappled with the more resolutely, in proportion as others shrank away from the task perplexed and wearied. I need not attempt any detailed view of his system: his First Class of Primary Stratified Rocks occupies the Highlands of Scotland and the Hebrides, and appears in Anglesea and Caernarvonshire; the crystalline slates of Skiddaw Forest, and the Upper Skiddaw slate-series come next. Above these is his Second Class, or Cambrian and Silurian System. The Cambrian is divided into Lower and Upper Cambrian, of which the former includes all the Welsh series under the Bala limestone; the two great groups of green roofing slate and porphyry on the north and south sides of the mineral axis of the Cumbrian Mountains (of which groups the position had previously been misunderstood), and parts of Cornwall and South Devon. The Upper

Cambrian system contains a large part of the Lammermuir chain; a part of the Cumbrian Hills, commencing with the calcareous slates of Coniston and Windermere; the system of the Berwyns and South Wales; all the North Devon, and a part of the South Devon and Cornish series. Ascending thus through a series of formations distinguished and reduced to order by the indefatigable exertions and wide views of Professor Sedgwick, we arrive at the Silurian system: and here we must seek our subdivisions from the rich results of the labours of Mr Murchison. These subdivisions were published in the summer of 1833. Like the Cambrian, the Silurian is divided into a Lower and an Upper system, the former including the Llandeilo flags and the Caradoc sandstones; the Upper Silurian rocks being the Wenlock shale and limestone, the Lower Ludlow, the Aymestry limestone, and the Upper Ludlow, which finally conducts us to the tilestones or bottom-beds of the old red sandstone.

That these various series of Cambrian and Silurian rocks are really superposed on one another; that they are justly separated into these groups; and that the smaller groups are truly of a subordinate nature, divided by lines less broad than those which bound the great series of formations;—these are points, of which the evidence must be sought in the works to which I refer. The evidence adduced by Professor Sedgwick is mainly to be found in the great fact of superposition, supported by the circumstances of dip, strike, cleavage, mineral character, and all the great incidents of mountain-masses. To proofs of this kind Mr Murchison is able to add the testimony of organic fossils, of which a vast and most instructive collection is figured in his work. These fossils of the Silurian system, amounting in all to about 350 species, are essentially distinct from those of the Carboniferous System and Old Red Sandstone. This being so, the establishment of these great divisions is supported by that geological evidence which properly belongs to the subject.

"In detecting order and system among the monuments of the most obscure and remote periods of the earth's history, it may easily be supposed that it has been necessary to employ and to improve all the best methods of geological investigation. Prof. Sedgwick's classification of the oldest rocks which form the surface of this island has of course been obtained by a careful attention to the position and superposition of the mineral masses, and by tracing the geographical continuity of the strata, almost mile by mile, from Cape Wrath to the Land's End. In this manner he has connected the rocks of Scotland with those of Cumberland; these again with those of Wales; and the Welsh series, though more obscurely, with that of Devonshire and Cornwall. In this survey he has constantly kept before his eyes a distinction, known indeed before, but never before so carefully and systematically employed, between the slaty cleavage of rocks and their stratification; for the directions of these two planes, though each wonderfully persistent over large tracts, never, except by accident, coincide. He has taken for his main guide the direction of the strata, or, as it is called, the *strike* of the beds; and in such a course, the theory of

Elie de Beaumont respecting the parallelism of contemporaneous elevations, whether true or false, could not fail to give an additional interest to geological researches, conducted on so large a scale as those of Prof. Sedgwick. Mr Murchison's mode of investigation may be described thus: that he has applied, for the first time, to the rocks below the old red sandstone, the method of classification previously employed with so much success for the oolites. It is truly remarkable, that Nature has placed in this our corner of the world, series, probably the most complete which exist, of both these groups of strata; and as the oolites of England have long been the type of that portion of European geology, the Silurians of Wales may perhaps soon be recognised as the standard members of a still more extensive range of deposits. As if Nature wished to imitate our geological maps, she has placed in the corner of Europe our island, containing an *Index Series* of European formations in full detail.

" The Carboniferous, Old Red, Silurian, and Cambrian systems have, by many writers, up to the present time, been all comprehended in the term " transition rocks," so far as that term has been used with any definite application at all. The analysis of this vague group into these distinct portions, removes the confusion and perplexity which have hitherto prevailed in this province of geology. Prof. Sedgwick has further proposed to apply the term *Palæozoic*; and Mr Murchison that of *Protozoic*, to the rocks which constitute the Cambrian and Silurian systems.

" How far these appellations are useful, we shall see when we have had speculations presented to us in which they are familiarly used; for necessity is the best apology, and convenience the best rule, of innovations in scientific language. In the names applied to the members of the Silurian system, Mr Murchison, following those examples of geological nomenclature which have been most clearly understood, and most generally adopted, has borrowed his terms from localities in which standard types of each stratum occur. If the Silurian system be as exclusively diffused as some indications seem to imply, we may find the Ludlow rocks in Scandinavia, and the Caradoc sandstone even in Patagonia. Whether a like identification of the more ancient rocks of the Cambrian series with the lowest formations of other countries be possible, may perhaps be (for the present) more doubtful.

" I have spoken of Mr Murchison's work as if it had formed part of our Proceedings, as, indeed, almost every part of it has done, although it now appears in a separate form. And I will add, that it is impossible not to look with pleasure upon the form in which the work appears, enriched as it is in the most liberal manner, with every illustration, map, and section, picturesque view, and well-marked fossil, which can aid in bringing vividly before the reader all the instructive and interesting features of the formations there described. The book must be looked upon as an admirable example of the sober and useful splendour which may grace a geological monograph.

" Having been tempted to dwell so long on this subject from my conviction of its importance, I must the more rapidly proceed with the remainder of my survey. Mr Bowman sent us 'Notes on a small patch of Silurian rocks to the west of Abergele.' In this investigation, which is interesting to us as the first application of Mr Murchison's Silurian system, the author found strata of which some could be, by means of fossils, identified with the Ludlow rocks. Mr Malcomson has, by the remains of fossil fishes, shewn that the calciferous conglomerate of Elgin represents the old red sandstone of Clashbinnie, as the Rev. G. Gordon had already supposed. Finally, proceeding to higher strata, we have to notice a trait of the fossil history of the coal-strata near Bolton-le-Moors, contributed by Dr Black. A stem of a tree thirty feet long, and inclined at an angle of 18° in a direction opposite to the strata, was discovered, having upon it a Sternbergia, about an inch in diameter, extending the whole length of the stem, which had been, while living, a parasite plant, like the mighty existing creepers of the tropical regions.

" The most curious addition to our fossil characters of strata, are the footsteps discovered on the surface of beds of the new red sandstone. It is well known that several years ago such marks were discovered at Corncockle Muir, in Dumfriesshire. Since that time, similar discoveries have been made at various places, and especially in 1834, in the quarries of Hesseberg near Hilbergshausen; and to the animal which had produced the impressions then discovered, the name of Chirotherium was provisionally applied by Professor Kaup. In the quarries of Storeton Hill, in the peninsula of Worral, between the Mersey and the Dee, marks were discovered strongly resembling the footsteps of the Chirotherium of Kaup: these were described by a committee of the Natural History Society of Liverpool, and drawn by J. Cunningham, Esq. Mr James Yates has also described footsteps of four other animals from the same quarries; and Sir Philip Egerton has given a description of truly gigantic footsteps of the same kind, which he terms the *Chirotherium Herculis*.

" Mr Strickland gave us a notice of some remarkable dikes of calcareous grit which occur in the lias-schist at Ethie in Ross-shire, and which had already been remarked by Mr Murchison, in his examination of the coast of Scotland, in 1826. They appear not to have been injected from below, but filled in from above.

" Mr Williamson's 'View of the Distribution of Organic Remains in part of the Oolitic Series on the coast of Yorkshire,' was the welcome continuation of a labour of the same kind already executed for the lower portions of the series, and promised to be continued for the upper. Among the contributions to the fossil history of the oolites, we must also place Dr Buckland's 'Discovery of the fossil wing of an unknown Neuropterous insect in the Stonesfield slate.' This stratum, the Stonesfield slate, has, during the past years, occupied the Society in the consideration of its fossils in no small degree; but the speculations thus suggested belong to Palæontology rather than descriptive geology. Mr

Murchison's notice of a specimen of the Oar's rock, which stands in the sea off the coast of Sussex, nine miles south of Little Hampton, shews it to agree with some of the rocks in the greensand or Portland beds; and its thus belonging to the strata below the chalk falls in with the remark of its occurring between the parallels of disturbance which traverse the Wealden of Sussex on the north, and the Isle of Wight on the south; for these disturbances and other facts agree well with the notion of protruded strata between. The wealden strata themselves have been observed by Mr Malcolmson, at Linksfield, near Elgin. It is remarkable, that these strata had already, very unexpectedly, been found by Messrs Murchison and Sedgwick in the Isle of Skye.

I have also to notice Dr Buckland's account of the discovery of fossil fishes in the Bagshot Sands at Goldworth Hill, near Guilford. As these fossils resemble those of the London clay, Mr Lyell's opinion that the Bagshot Sands were deposited during the eocene period is strongly confirmed.

The fresh-water beds of the Isle of Wight, which had already supplied specimens of some of the Pachydermata of the Paris basin, have furnished an additional supply of rich fossils, which have been examined by Mr Owen. He has found them to contain bones of four species of Palæotherium, and two species of Anoplotherium; also a jaw of the Chæropotamus, a fossil genus established by Cuvier; and another jaw closely resembling that of a Musk Deer, which Mr Owen refers to the genus Dicobune, a genus also established by Cuvier upon the fossils of the Paris basin. Such discoveries falling in with the conclusions obtained by the researches of previous philosophers respecting the tertiary period of the earth's history, and supplying what they left imperfect, cannot fail to give us great confidence in the results of those investigations, and to enhance our admiration of the sagacity which opened to us this path of discovery.

Dr Mitchell gave an account of his attempts to trace the drift from the chalk, and strata below the chalk, as it exists in the counties of Norfolk, Suffolk, Essex, Cambridge, Huntingdon, Bedford, Hertford, and Middlesex. This drift I had occasion to notice in my Address last year, in reference to Mr Clarke's elaborate geological survey of Suffolk; and I then stated that this diluvial deposit is known in the neighbourhood of Cambridge by the name of *brown clay*. Dr Mitchell has shewn that this deposit is of greater extent than we were before aware. But still to determine with precision its principal masses, total extent, and local modifications, would be a valuable service to the geology of the eastern part of our island.

As my order requires me to take the igneous after the sedimentary rocks, I must here notice Dr Fleming's "Remarks on the Trap Rocks of Fife," which he distinguishes into three epochs;—those of the eastern extremity of the oolites, which are variously associated with the old red sandstone;—those which run from St Andrews to Stirling, which were produced after the coal-measures;—and those which occur along the shores of the Forth, which occur in the higher coal-measures.

2. *Foreign (South European and Trans-European) Geology.*—In the survey of the progress of our labours which I offered to your notice last year, I stated, that, in proceeding beyond the Alps, and I might have added the Pyrenees, we no longer find that multiplied series of strata, so remarkably continuous and similar, when their identity is properly traced, with which we have been familiar in our home circuit. Yet the investigations of Mr Hamilton and Mr Strickland appear to shew, that we may recognise, even in Asia Minor, the great formations, occupying the lowest and highest positions of the series, which are well marked by fossils, namely the Silurian and Tertiary formations; and also an intermediate formation corresponding in general with the secondary rocks of the north, but not as yet reduced to any parallelism with them in the order of its members. Besides these sedimentary rocks, in this, as in most other countries, there are found vast collections of igneous rocks of various kinds, which interrupt and modify, and may mask and overwhelm the fossiliferous strata. A paper has been communicated to us by Mr Hamilton, "On a part of Asia Minor," namely, the country extending from the foot of Hassan Dagh to the great salt lake of Toozla, and thence eastwards to Cæsarea and Mount Argæus, and thus occupying a part of the ancient Cappadocia.

It appears that in this district the igneous rocks occupy a large portion of the surface, and the sedimentary strata which are associated with these are not easily identified with those which occur in countries already examined. The district examined by Mr Hamilton contains a limestone belonging to the vast calcareous lacustrine formation of the central part of Asia Minor, and beneath this, a system of highly-inclined beds of red sandstone, conglomerates and marls, which are perhaps connected with the saliferous deposits of Pontus and Galatia; but which could not be satisfactorily compared with the beds of the south of Europe, for want of the occurrence of organic remains. In only one instance did Mr Hamilton observe the trace of organic bodies in the sandstone; these were impressions resembling fucoids, and similar to those found in the Alpine limestone near Trieste. Mr Hamilton ascended to the summit of Mount Argæus, which had not previously been reached by any traveller, which rises abruptly from the alluvial plain of Cæsarea to the height of 13,000 feet.

We have another contribution to the geology of the countries exterior to the Alps and Pyrenees in Mr Sharpe's memoir on the geology of Portugal. He has examined with great care the neighbourhood of Lisbon, and has traced the superposition of the strata, naming the most conspicuous of them from the places in which they are well exhibited. His series (exclusive of igneous rocks) consists of San Pedro limestone (which rests upon the granite), slate-clay and shale, Espichel limestone, red sandstone, hippurite limestone, a lower tertiary conglomerate, the Almada beds, and the upper tertiary sand. In the Memoirs of the Royal Academy of Sciences of Lisbon, for 1831, Baron Eschwege had examined a geological section taken across the mouth of the Tagus, and passing from the granite of the Serra of Cintra, to that of the Serra of Arrabida. But his identifications of the Portuguese beds do not agree with those of Mr Sharpe, and have indeed the air of proceeding on the arbitrary assumption of a correspondence between this and other parts of Europe. Thus Baron Eschwege has referred both the San Pedro and the Espichel limestones to the magnesian limestone; the red sandstone-formation he considers as Bunter Sandstein, while Mr Sharpe refers it to the age of our Oolites: the hippurite limestone (now acknowledged to be the equivalent of our chalk and greensand), M. Eschwege makes to be Jura limestone; and the Almada beds he would have to be Plastic Clay and Calcaire Grossier. Mr Sharpe is very properly attempting, by a further study of the organic fossils which he has procured, to confirm or correct the identifications to which he has been led. It is only by thus starting from different points, and tracing strata by their continuity, that we can hope to cover the map of Europe, and finally the world, with geological symbols of a meaning fully understood.

Palæontology.

" The portion of our subject which we term Palæontology might, at first sight, seem to form a part of zoology rather than of geology; since it is concerned about the forms and anatomy of animals, and differs from the usual studies of the zoologist only in seeking its materials in the strata of the earth's crust istead of upon its surface. Yet a moment's thought shews us how essential a part of our science the zoology of extinct animals is; for, in order to learn the history of the revolutions which the earth has undergone, we must seek for general laws of succession in the remains of organic life which it presents, as well as in the position and structure of its brute masses. And since such general laws must necessarily be expressed in terms of zoology, it becomes our business to define those terms, so that they shall be capable of expressing truths which include in their circuit the past as well as the present animal and vegetable population of the world.

" An example of this process has occupied a large portion of our attention during the past year. It appeared to be a proposition universally true, that the oldest strata of the earth's surface contained cold-blooded animals only; and that creatures of the class mammalia only began to exist on the surface after the chalk strata had been deposited and elevated. And when, to a rule of this tempting generality, a seeming exception was brought under our notice, it became proper to examine, whether the anatomical line, which enables us to separate hot-blooded from cold-blooded animals, had really been rightly drawn; and whether, by rectifying the supposed characteristic distinction, the exception might not be eliminated. The exception, on which this very instructive point was tried, consisted in a few jaw-bones of a fossil animal, which, though occurring in the Stonesfield slate near Oxford, a bed belonging to the oolite formation, had been referred by Cuvier to the genus Didelphys, and thus placed among marsupial mammals. In August last, M. de Blainville stated to the Academy of Sciences of Paris his reasons for doubting the justice of the place thus assigned to the fossil animal. Founding his views principally upon the number and nature of the teeth of the fossil, he asserted that the animal, if a mammal, must come nearest the phocæ; but he rather inclined to believe it a saurian reptile; following, as he conceived, the analogies offered by a supposed fossil saurian described by Dr Harlan of Philadelphia, and termed by him Basilosaurus. M. Valenciennes, on the other hand, asserted the propriety of the place assigned by Cuvier to the fossil animal, although he made it a new genus; and gave to the species the name *Thylacotherium Prevostii.* The controversy at Paris had its interest augmented when Dr Buckland, in September, carried thither the specimens in question. From Paris the controversy was transferred hither in November, and principally occupied our attention at our meetings till the middle of January.

" One advantage resulting from the ample discussion to which the question has thus been subjected, has been, that even those of us who were previously ignorant of the marks by which zoologists recognise such distinctions as were in this case in question, have been put fully in possession of the rules and the leading examples which apply to such cases. And hence it will not, I trust, be deemed presumptuous, if, without pretending to any power of deciding a question of zoology, I venture to state the result of these discussions. It appears, then, that some of the marks by which the under jaws of Mammals are distinguished from those of Saurians are the following: (1) a convex condyle; (2) a broad and generally elevated coronoid process; (3) rising near the condyle; (4) the jaw in one piece; (5) the teeth multicuspid, and (6) of varied forms, (7) with double fangs; (8) inserted in distinct sockets, but (9) loose and not anchylosed with the jaw. In all these respects the Saurians differ; having, for instance, instead of a simple jaw, one composed of six bones with peculiar forms and relations, and marked by Cuvier with distinct names; having the teeth with an expanded and simple fang, or anchylosed in a groove, and so on. Of course, it will be supposed, by any one acquainted with the usual character of natural groups, that this line of distinction will not be quite sharp and unbroken, but that there will be apparent transgressions of the rule, while yet the unity of the group is indubitable. Thus the Indian Monitor and the Inguana, though Saurians, violate the *second* character, having an elevated coronoid process; but then it is narrow, and this seeming defect in our second character is further remedied by the third; for in those Saurians there is a depressed space between the condyle and the coronoid process quite different from that which a mammal jaw exhibits. Again, the teeth of Crocodiles, Plesiosaurs, and the like, are inserted in distinct sockets; but then they have not double fangs. The Basilosaurus was supposed to be a saurian with double-fanged teeth, but that exception was disposed of afterwards. And as there are thus saurians which trench upon the characters of mammals, there are mammals in which some of the above characters are wanting: thus the condyle is slightly or not at all convex in the Ruminantia; there is no elevated coronoid process in the Edentata; the Dolphin and Porpoise have not multicuspid teeth; the Armadillo has not varied forms of teeth, nor has it double fangs to its teeth, which also the fossil Megatherium has not. Still, upon the whole, the above appears to be the general line of distinction. Even if one or two of the above nine marks were wanting to prove the animal a mammal, still if the great majority of them were present, our judgment could not but be decided by the preponderance of characters. But if all the above characters of mammals are present, and all those of saurians absent, it seems to be a wanton scepticism to doubt that the animal was really warm-blooded.

" Now it was asserted by Mr Owen, who brought this subject before us, that this is the case; that all the characters which I have enumerated above exist in the Stonesfield jaws. If we satisfy ourselves that this is the case, I do not see how we can avoid assenting to his opinion,—that the animal belonged to the class Mammalia.

" Every such question of classification must resolve itself into two; that of the *value*, and that of the *existence* of the characters. If we assent to Mr Owen in his view of the former, we are then led to consider the latter.

" M. de Blainville, at least in his first examination, had laboured under the disadvantage of forming his judgments from casts and drawings only of the Stonesfield bones. Under these circumstances, he had denied several of the above characters; he had held that the teeth in the Thylacotherium are uniform; and that they are confluent with the jaw; and that the jaw is compound. These statements Mr Owen, resting upon a careful examination of the specimens, contradicts. The assertion of the compound nature of the jaw is occasioned by a groove near the lower margin of the jaw, which, however, is not so situated as to represent the saurian sutures, but is completely explained by supposing it to be a vascular canal, such as exist in the Wombat, Didelphys, Opossum, and similar animals.

" Another specimen, at that time the property of Mr Broderip, but now very properly placed in the British Museum, exhibits a jaw similar indeed to the Thylacothere, but belonging to a different genus; and to this species Mr Owen has given the name *Phascolotherium Bucklandi*. But these generic names imply that the animals are pouched animals; and in addition to the reasons which led Cuvier to this opinion, Mr Owen has noticed in the fossils an inflection of the lower edge of the jaw, which, so far as has been hitherto observed, occurs in Marsupials, and in them alone.

" As if this question had been destined to be settled at this time, the only remaining doubt with regard to the possible existence of double fangs in the teeth of a saurian was removed by the arrival in London of Dr Harlan with his 'Basilosaurus.' That gentleman, with great liberality and candour, allowed sections of the fossil to be made in such a manner as to expose the structure of the teeth. And these being examined by Mr Owen, and compared with the general laws of dental structure which he has lately discovered, it appeared that Dr Harlan's fossil was by no means a saurian, but an animal nearly allied to the Dugong, to which Mr Owen proposes to apply the generic name of Zeuglodon, expressing the conjoined form of its teeth.

" I have not hesitated to lay before you the view of this subject to which I have been led by the discussions in which we have been engaged, notwithstanding the very great authorities which incline to the other side of the balance. Among these I hardly know whether I am to reckon Mr Ogilby, who laid before us a very instructive communication, in which, without deciding the point, he pointed out the difficulties which appear to him to embarrass both views, and especially to contradict the opinion of the marsupial nature of the animal.

" I have dwelt the longer on this controversy, since it involves considerations of the most comprehensive interest to geologists, and, we may add, of the most vital importance. For—*de summa reipublicæ agitur*,—the battle was concerning the foundations of our philosophical constitution; concerning the validity of the great Cuvierian maxim,—that from the fragment of a bone we can reconstruct the skeleton of the animal. This doctrine of final causes in animal structures, as it is the guiding principal of the zoologist's reasonings, is the basis of the geologist's views of the organic history of the world; and, that destroyed, one-half of his edifice crumbles into dust. If we cannot reason from the analogies of the existing, to the events of the past world, we have no foundation for our science; and you, gentlemen, have all along been applying your vigorous talents, your persevering toil, your ardent aspirations, idly and in vain.

" Besides the important investigations thus referred to, we owe to Mr Owen other palæontological contributions. The genus *Chæropotamus*, established by Cuvier from an imperfect fragment of the bone of a skull, was asserted by him to be a Pachyderm most nearly allied to the Peccari. A fragment of a lower jaw of the same genus, found by Mr Darwin Fox in the Isle of Wight, confirms this view, but indicates in some points an approach to the carnivorous type. And it was remarked as interesting, that the living genus of the hog tribe which most resembles the Chæropotamus, the Peccari, exists in South America, where the Tapir, the nearest living analogue of the Anoplothere and Palæothere, the associates of the Chæropotamus, also occur. Another jaw, found by Mr Pratt in the Binstead quarries in 1830, and resembling that of the Musk Deer, Mr Owen refers to a new species of Cuvier's genus Dicobune, under the name *Dichobune cervinum*. Mr Owen has also given us a description of Lord Cole's specimen of *Plesiosarus macrocephalus,'* which he compares with Mr Conybeare's *Plesiosaurus Dolichodeirus,* by establishing an intermediate species, founded upon a specimen existing in the British Museum, and termed by him *Plesiosaurus Hawkinsii.* Besides tracing the analogies which connect these with each other, and comparing them with the two great modifications of the saurian tribe, the crocodiles and the lizards, Mr Owen presented his remarks on the form of the Plesiosaurian vertebræ, founding them upon a general view of the elements of which all vertebræ are constituted.

"To the communications thus made to us, we may add Mr Owen's determination of another animal, of which the remains brought from the neighbourhood of Buenos Ayres, are among the many treasures of this kind which we owe to Sir Woodbine Parish. This animal, of gigantic dimensions, appears to have been allied to the Megatherium, but with closer affinities to the Armadillos; and it probably possessed the characteristic armour, of which, in the Megatherium, the existence is perhaps problematical. Mr Owen has termed it *Glyptodon,* from the furrowed shape of its teeth.

"In another communication Mr Owen endeavoured to account for the dislocation of the tail of the Ichthyosaurus at a certain point, which is observable in many of the fossil skeletons of that animal. This circumstance, so remarkable from its general occurrence, and which Mr Owen was the first to observe, he is disposed to account for, by supposing a broad tegumentary fin to have been attached to the tail for a portion of its length, the position of which fin must, he conceives, have been vertical.

"I cannot close my enumeration of the valuable contributions for which we are indebted to Mr Owen, without remarking how well our anticipations have been verified, when, in awarding him the Wollaston medal last year, we considered the labours which we thus distinguished as only the beginning of an enlarged series of scientific successes; and how well also Mr Owen's own declaration, that he should lose no available time or opportunity which could be applied to palæontological research, has been borne out by the services he has rendered that branch of our science.

"In the remainder of my review of what has been done among us in Palæontology I must necessarily be very brief. I have already mentioned the discovery of fossil fishes in the Bagshot sand. These fishes have supplied three new genera, which Dr Buckland has distinguished and has named *Edaphadon, Passalodon,* and *Ameibodon;* of which the two first offer combinations of the characters of bony and cartilaginous fishes. Mr Stokes has given us his views of the structure of the animal to which belonged those fossils with which we are so familiar under the name of Orthoceratites. He is of opinion, that these fossils, in their living condition, existed as a shell, enveloped within the body of the animal to which they belonged. He has distinguished three genera of these shells, to which he assigns the names *Actinoceras, Ormoceras,* and *Huronia.* The Marquis of Northampton also has examined those minute spiral shells which occur in the chalk and chalk flints, and have been termed Spirolinites. And, finally, under this head I must mention Mr Alfred Smee's paper on the state in which animal matter is usually found in fossils.

"Mr Austen's hypothesis of the origin of the limestone of Devon, though belonging in some measure to Geological Dynamics, may perhaps be mentioned here, since he explains the position of those beds by reference to the habits of the coral animal. Mr Austen has already shewn himself to us as an excellent observer; and in constructing geological maps, a task requiring no ordinary talents and temper, he has earned our admiration. We shall therefore not be thought, I trust, to depreciate his labours if we receive with less confidence speculations in their nature more doubtful. As we can hardly suppose the calcareous beds of Devon to have had an origin different from those of other countries, we cannot help receiving with some suspicion a doctrine which would subvert almost the whole of our existing knowledge of the relations of fossiliferous beds of limestone.

Geological Dynamics.

"In that part of geology which I have termed Geological Dynamics, and which investigates and applies those causes of change by which we may hope to explain geological phenomena, we may still observe that fundamental antithesis of opinion which has so long existed on the subject:—the division of our geological speculators into *Catastrophists* and *Uniformitarians*;—into those who read in the rocks of the globe the evidence of vast revolutions, of an order different from any which those of man has survived:—and those who see in the condition of the earth the result of a series of changes which are still going on without decay, the same powers which produced the existing valleys and mountains being yet at work about us. Both these opinions have received their contributions during the preceding year: Mr Darwin having laid before us his views of the formation of mountain-chains and volcanos, which he conceives to be the effect of a gradual, small, and occasional elevation of continental masses of the earth's crust; while Mr Murchison gathers from the researches in which he has been engaged, the belief of a former state of paroxysmal turbulence, of much deeper rooted intensity and wider range than any that are to be found in our own period; and M. de Beaumont in France, has endeavoured to prove that Etna and many other mountains must have been produced by some gigantic and extraordinary convulsion of the earth. Both Mr Darwin and M. de Beaumont refer to the same examples; and while M. de Beaumont conceives that the cones of the Andes must have been formed by an abrupt elevation, caused by subterranean force, Mr Darwin has maintained the opinion, that these lofty summits have been gradually thrust into the place which they occupy by a series of successive injections of molten matter from below, each intruded portion of fluid having time to harden into rock before it was burst and again injected by the next molten mass. For how otherwise, he asks, can we conceive the strata to be thrust into a vertical position by a liquid from below, without the very bowels of the earth gushing out? Without attempting to answer this question, we may observe, that when we suppose, as Mr Darwin supposes, a vast portion of the earth's crust, the whole territory of Chili for example, to rest on a lake of molten stone, there is considerable force in M. de Beaumont's argument:—that when such a fluid is raised to the top of a mountain ten or twenty thousand feet high, the pressure upon the crust which is in contact with the fluid must be more than a thousand atmospheres; and who, *he* too asks, flatters himself that he knows enough of the interior machinery of volcanos, to be certain that this vast pressure, acting upon a large surface, may not, by some derangement of its safety-valve, the volcanic vent, produce effects to which we cannot assign any limit?

In speaking of Mr Darwin's researches, I cannot refrain from expressing for myself, and I am sure I may add for you, our disappointment and regret that the publication of Mr Darwin's journal has not yet taken place. Knowing, as we do, that this journal contains many valuable contributions to science, we cannot help lamenting, that the customs of the Service by which the survey was conducted have not yet allowed this portion of the account of its results to be given to the world.

Although not communicated to us, but to our Alma Mater the Royal Society, I may notice Mr Hopkins's endeavours to throw light upon such subjects as this by the aid of mathematical reasoning. The researches of Mr Hopkins respecting the effects which a force from below would produce upon a portion of the earth's crust have already interested you, and would be of still greater value, if the directions of faults and fissures which result from his theory did not depend very much upon that which, in most cases, we cannot expect to know, the form of the area subjected to such strain. Mr Hopkins has since been employing himself in tracing the consequences of another idea, truly ingenious and philosophical, and which a person in full possession of the resources of mathematics could alone deal with. Some of the effects which the sun and moon produce upon the earth (as the precession and nutation,) include the attraction of those bodies upon the interior portion of the earth, and have hitherto been deduced from the theory by mathematicians, upon the supposition that the earth is solid. But what if the central portion of the earth were fluid? What if it appeared, by calculation, that the fluid internal condition would make the amount of the precession of the equinoxes, or of the nutation of the axis, different from that which the solid spheroid would give? What if it appeared, that the precession and nutation thus calculated for a fluid interior agreed better with observation than the result hitherto obtained by supposing the earth solid? If this were so, we should have evidence of the earth's interior fluidity, evidence, too, of a perfectly novel and striking nature. But to answer these questions is far from an easy task; the precession of the solid earth is a problem in which Newton erred, and in which the greatest mathematicians of modern times have not found their greatest strength superfluous. Yet how incomparably more difficult in all cases is the mechanics of fluid than of solid bodies! It may, therefore, require more than one trial before any satisfactory solution of the problem can be obtained. Mr Hopkins has attacked it by the aid of certain hypotheses, and the result is, so far, not favourable to the decisiveness of this test of the interior condition of the earth; but, notwithstanding this state of things, I venture to say on your behalf, gentlemen, that an idea so full of promise of that which we so much desire, and which seems to be so utterly out of our reach, the knowledge of the condition of the centre of the earth,—that such an idea is not to be slightly abandoned.*

M. Necker, of Geneva, offered an addition to the causes of convulsions of the earth, which are contemplated by our Geological Dynamics, in a paper in which he ascribed the earthquakes which took place in the southern provinces of Spain, in 1829, to the falling in of strata, the subjacent gypseous and saliferous masses being washed out by subterraneous currents. Without denying all influence to such a cause, we may observe, that it does not appear likely that there would be thus produced, simultaneously, any greater effects than those which are known to have occurred from the falling in of unsupported mines; and these have never approached in their scale to any except the smallest earthquakes.

While geologists are thus looking in all directions for causes which may produce the phenomenon which they study, it is natural that the powerful, but as yet mysterious, influences of electricity should draw their attention. Mr Robert Were Fox has endeavoured to shew, that by voltaic agency, a laminated structure, and deposits of metals in cracks, resembling metallic veins, may be produced in masses of clay. The experiments are of an interesting kind, and it can hardly be doubted that voltaic agency had some influence in such cases as those described by Mr Fox; although Mr Henwood and Mr Sturgeon have failed in attempting to reproduce his results, and although results much resembling these occur in cases where no electrical action is suspected. But we may remark, that the conditions under which such voltaic effects are produced, have not yet been attempted to be defined with any accuracy; and that, till this is done, the reality of such agency can neither be verified nor applied to geological speculations.

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* The following are the results at which Mr Hopkins has arrived, supposing the earth to consist of a homogeneous spheroidal shell filled with a fluid mass of the same density as the shell:—

1. The precession will be the same, whatever be the thickness of the shell, as if the whole earth were solid.

2. The lunar nutation will be the same as for the solid spheroid, to such a degree of approximation, that the difference would be inappreciable to observation.

3. The solar nutation will be sensibly the same as for the solid spheroid; unless the thickness of the shell be very nearly of a certain value, something less than one-fourth the earth's radius, in which case this nutation might become much greater than for the solid spheroid.

4. In addition to the above motions of precession and nutation, the pole of the earth would have a small circular motion, depending entirely on the internal fluidity. The radius of the circle thus described would be the greatest when the thickness of the shell should be least; but the inequality thus produced would not, for the smallest thickness of the shell, exceed a quantity of the same order as the solar nutation; and for any but the most inconsiderable thickness of the shell, would be entirely inappreciable to observation.

Mr Hopkins intends hereafter to consider the case of variable density.

A reflection which naturally offers itself upon this review of our recent career, is this:—that different portions of the science of geology advance with very different rapidity. Descriptive Geology is constantly and actively progressive: facts are accumulated by observers in every land; and though facts are, in truth, of no value, at least for any purpose of science, except so far as they are reduced to some classification, yet, on the other hand, sound classifications are perpetually, almost necessarily, suggested, when observation is vigilant and persevering. Even if we at first express our facts in terms of a false classification, we find afterwards the means of translating them into the language of a true one. And the spirit of geological observation is so widely diffused, and so thoroughly roused, that I trust we need not anticipate any pause or retardation in the career of Descriptive Geology. I confess, indeed, for my own part, I do not look to see the exertions of the present race of geologists surpassed by any who may succeed them. The great geological theorizers of the past belong to the *Fabulous Period* of the science; but I consider the eminent men by whom I am surrounded as the *Heroic Age* of geology. They have slain its monsters, and cleared its wildernesses, and founded here and there a great metropolis, the queen of future empires. They have exerted combinations of talents which we cannot hope to see often again exhibited, especially when the condition of the science which produced them is changed. I consider that it is now the destiny of geology to pass from the heroic to the *Historical Period*. She can no longer look for supernatural successes, but she is entering upon a career, I trust a long and prosperous one, in which she must carry her vigilance into every province of her territory, and extend her dominion over the earth, till it becomes, far more truly than any before, an universal empire.

Such are the prospects of Descriptive Geology;—of the geology of facts and classifications. To our knowledge of causes, we can look with no such certainty of its progress being steady and rapid; or rather, we are certain the advance must be slow, and may be often and long interrupted. For it is not an advance, to suggest one or another hypothetical cause or change, without assigning the laws and amount of the change: it is hardly an advance even to calculate the results of our hypothesis on assumed conditions. To obtain by induction, from adequate facts, the laws of change of the organic and inorganic creation,—this alone can lead us to those discoveries which must form the epochs of Geological

Dynamics. And we have yet to learn, whether man's past duration upon the earth, whether even that which is still destined to him, is such as to allow him to philosophize with success in such matters;—whether, not individuals only, not a generation alone, but whether the whole species be not too ephemeral, to penetrate, by the unassisted powers of its reason, into the mystery of its origin:—whether man, placed for a few centuries on the earth as in a school-room, have time to strip the wall of its coating, and count its stones, before his parent removes him to some other destination.

And now, gentlemen, I approach the close of my task, and of the office which has been imposed upon me; an office which has been to me a source of unmingled gratification. The good opinion implied by your selection of me, the good opinion of such a body of men, was an occasion of sincere and earnest self-congratulation,--a self-congratulation hardly damped by my consciousness of an imperfect acquaintance with your science;—since I trusted that you, though not unaware of my defects, had judged that good will, and a disposition to look at the subject in its largest aspect, might in some measure compensate for them. And if I needed other grounds of satisfaction in the employment which I am thus bringing to its close, I might find them in the reflections I have just been led to make in the progress and prospects of the science with which you are concerned. For it has ever been one of my most cherished occupations, and will, I trust, long be so, to trace the principles and laws by which the progress of human knowledge is regulated from age to age in each of its provinces. To have had brought familiarly under my notice, in a living form, the daily advance of a science so large and varied as yours, has been, as it could not but be, a permanent and most instructive lesson;—perpetually correcting lurking mistakes, and suggesting new thoughts. And if, while I have looked at your science in this spirit, you have thought me worthy to be called to preside over your body for two years; and if, during that time, you have not repented of your choice, as I have not found my views inapplicable to the subjects which have come before you; I may, I would believe, find in this some ground for confiding in the trains of thought which have thus led me to such a position; and may hope that, however arduous be the task of framing a philosophy of science suitable to its present condition, and of using such a philosophy as a means of furthering knowledge in general, still, that in this task, to which our age is so manifestly called, I too may be a helper.

I trust that you will excuse these few words uttered with reference to my own peculiar pursuits, since these include yours also, and are my only claim to your indulgence. And now, gentlemen, that I may trespass upon that indulgence no longer, I once more thank you in all earnestness and sincerity for your good opinion which placed me in this chair, and for the kindness and support which I have on all occasions received from you; and with my best wishes for your prosperity, and that of your science, I resign my office into abler hands.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany.

10*th June* 1839.

Anadenia Manglesii.

A. *Manglesii;* foliis cuneiformibus, tripli-nervibus, venosis, utrinque glabris et subconcoloribus, trifidis; lobis cuneiformibus, lateralibus 3-fidis, centrali 3–5-fido, segmentis mucronatis.

DESCRIPTION.—*Shrub* erect. *Branches* scattered, spreading, as well as the trunk round, green, slightly pruinose. *Leaves* (1¾ inch long, and as much broad, scattered, rigid, spreading, cuneate, and tapering into a slender petiole, 3-lobed, green on both sides, paler and brighter below, triple-nerved, veined; the lobes cuneate, 3-fid towards the apices, the centre lobe sometimes 5-cleft, each segment terminated with a rigid mucro; nerves hard and prominent behind, flat in front. *Racemes* (about 1½ inch long) terminal, or, from appearing on very short branchlets, seeming to be axillary, many-flowered; rachis always straight; pedicels (½ an inch long) white, very slender, at first erect, afterwards spreading wide, or arching backwards. *Flowers* in pairs, springing from the axil of a single bractea, the lowest spreading first. *Perianth* (about 3 lines long) glabrous, tetraphyllous, at first opening between the claws, and adhering at the limb, which in this stage is green and capitate, afterwards reflected when the limb becomes yellow, concave. *Anthers* sessile in the limb of the calyx, yellow, lobes slightly divaricated at the base; pollen granules minute, white, 3-angular. *Pistil* stipitate, and with its white glabrous footstalk as long as the perianth, prominent, and exposed when this is reflected. *Stigma* pink, conical. *Style* white, tumid in front. *Germen* yellow, projecting in front. *Ovules* 2.

We received this plant at the Botanic Garden, Edinburgh, from Mr Lowe of the Clapton nursery, in 1837, under the name of *Grevillea Manglesii.* The flowers are small, but being very numerous, covering the whole plant, and remaining long in perfection, it is far from being destitute of beauty. It first came into flower in the greenhouse, under the ordinary treatment of New Holland plants, in March, and now (31st May), though the spikes of expanded flowers are clustered at the apex of every branch, there is a long succession of undeveloped buds.

Bongardia, *Meyer.*

GENERIC CHARACTER.—Sepala 3–6: externe nuda. Petala 6, intus basi nuda, poro nectarifero instructa. Stylus disco foliaceo reniformi plicato margine stigmatoso terminatus. Fructus inflatus, membranaceus, unilocularis, indehiscens, 2–4 spermus. Semina subglobosa, fundo fructus inserta. Herbæ acaules, glaberrimæ, glaucæ, radice tuberosa; foliis omnibus radicalibus, pinnatisectis; scapis apice ramosis, ad ramorum basin squamosis; floribus aureis.

B. *Rauwolfii;* foliorum segmentis cuneatis, semiverticillatis, utrinque geminis.—*Meyer.*

Bongardia Rauwolfii, *Meyer,* Enumeratio Plantarum in Mont. Caucas. et in Provinc. Caspiis collect. p. 174.

Leontice Chrysogonum, *Lin.* Hort. Cliff. 122.—*Ibid.* Sp. Pl. i. 447.—*Willd.* Sp. Pl. ii. 148.—*Pers.* Synops. i. 386.—Hort. Kew. ii. 272.—*DC.* Syst. Nat. ii. 24 !—*Ibid.* Prodr. i. 109 !—*Sprengel,* Syst. Veget. ii. 121.—*Schultes,* Syst. Veget. vii. 20.—*Henslow,* in the Botanist, t. 50.

DESCRIPTION.—*Tuber* round, reticulated. *Leaves* all radical, verticillato-pinnate; leaflets elliptical, concave on the upper side, cuneate at the base, where, on the upper side, there is a large brown mark, every where else glaucous, frequently incised at the apex. *Scape* erect, paniculate, round, glaucous, spotted with brown towards the base; branches erect, subtended by oblongo-ovate, membranous brown bracteæ. *Flowers* terminal. *Calyx* of six leaflets, yellowish-green, glabrous on both sides, imbricated, caducous, the outer segments bright red at the edges. *Corolla* yellow, of six spreading, obovate, rather distant petals, imbricated in the bud, concave, slightly crisped at the apex, each with a nectariferous pit at its claw, and a corresponding gibbosity on the outside. *Stamens* six, hypogynous, erect, opposite the petals; filaments short; anthers longer than the filaments, curved inwards, and flattened on the inner side, the lobes round and prominent on the back, and there having a deep groove between them, opening by the rolling up of lateral valves, connective blunt and projecting beyond the anther lobes; pollen pale orange, granules small, elliptical. *Pistil* rather longer than the stamens, nearly half as long as the petals; style adherent to the germen from the base to the apex of this, and almost immediately on becoming free it is dilated and plicate; stigma marginal; germen green, membranous, inflated, 1-celled, ovate, compressed, having many narrow vertical membranous wings. *Ovules* several (about 9), erect, stipitate, arising from the base of the cell.

This very curious and interesting plant, so much resembling *Leontice*, yet so easily distinguished from it by the generic character quoted above (and for the transcript of which I am indebted to Sir William Hooker, not being able at the time to see the work in which it is published), flowered in the greenhouse of Dr Neill, Canonmills, Edinburgh, in the middle of April last. A tuber was received by him from Dr Fischer, St Petersburgh, three years ago; which, when planted on the surface, shewed only leaves; but on being immersed two inches in the soil, threw up a flower-stem. It is said by Meyer to abound in the hills, plains, and fields to the westward of the Caspian, and that the tubers, either roasted or boiled, are eaten by the Persians. Linnæus and subsequent writers tell us that it grows in corn-fields in Greece, and Sprengel and Schultes add Syria and Asia Minor. I am indebted to Professor Don for the reference to Hortus Kewensis, which I had overlooked. It appears from this that the plant was in cultivation in this country a hundred years ago, but it had certainly been lost, till now again introduced by Dr Fischer. "The plant found by Olivier in the island of Scio, Meyer considers a distinct species, which he calls *B. Olivieri*; and he supposes it to be what De Candolle (Syst. ii. 24.) has described as *Leontice chrysogonum*."—*Don*, MS. It does not seem unlikely that the plant in all the European stations may belong to this species, and that it may be distinguished from the plant on the shores of the Caspian by the much greater length of its filaments.

Epimedium Musschianum.

E. *Musschianum*; foliis ternatis; floribus squalide albidis, petalis calycem superantibus; stylo filiformi subcentrale, stigmate sublobato.

Epimedium Musschianum, *Morren et Decaisne*, Ann. Sc. Nat. 2de serie, tom. ii. p. 353.

DESCRIPTION.—*Petioles* suberect, trifid, swollen at the point of subdivision, as well as its branches round; *barren ones* (in the specimen described) glabrous and naked at the base, their subdivisions monophyllous, half as long as the petiole, leaflets cordate, glabrous, 7-nerved, bright green above, subglaucous below; *flower-bearing ones* twice as long as the others, sheathed at the base by several equitant, inflated, glabrous, coloured, 3-nerved stipules, loosely clothed with spreading tomentum, most abundant at its subdivision and at the origin of the panicle, branches unequal, the central the longest, but less than half as long as the petiole, 3-foliolate; leaflets very hairy on the lower side, at least when young. *Peduncle* (2½ inches long) arising from the petiole (about an inch) below its subdivision, rather less hairy than the petiole or its branches, suberect, bearing the pedicels, which are subglabrous, secund, and often bipartite near its apex. *Bracteæ* small, subulate, adpressed, solitary below the origin of the pedicels and their subdivisions, and six imbricated adpressed upon the sides of the calyx, sprinkled with red spots, deciduous. *Flowers* white, drooping, the upper expanding before the lower. *Calyx* spreading or suberect, reflexed, sepals rhomboideo-lanceolate, marked obscurely on the back by several parallel colourless ribs. *Petals* erect, blunt, involute at the edge, together forming a short cylinder in the centre of the flower, each having a short, spreading, subulate, compressed spur, reaching to about the middle of the sepal. *Stamens* yellow, erect, half as long as the petals; filaments very short; anthers elongated, subulate, 2-lobed, opening by lateral valves rolling up from the base; connective pointed. *Pistil* scarcely longer than the stamens; germen oblong; style lateral, about as long as the germen, green; stigma cupped, fringed.

We received this plant at the Botanic Garden, Edinburgh, from Mr Young of Epsom in 1838. It flowered for the first time in the greenhouse in March 1839, but will, without doubt, bear cultivation in the open border. It is one of the many acquisitions from the flora of Japan which European botanists owe to M. Von Siebold.

Epimedium violaceum.

E. *violaceum*; foliis triternatis; pedicellis subunifloris; floribus violaceis; petalis calycem superantibus; stylo filiformi sublaterali.

Epimedium violaceum, *Morren et Decaisne* in Ann. Sc. Nat. 1834, p. 354, t. 12.

Description.—*Barren petiole* filiform, wiry, glabrous, trifid, swollen at the joint, subdivisions nearly as long as the petiole, swollen at their apices, spreading, each (in the specimen described) supporting one obliquely cordate subacuminate leaflet, which (at least when old) is glabrous on both sides, spinuloso-ciliate. *Fertile petioles* reddish-brown, nearly twice as long as the others, clothed with spreading hairs both above and below the origin of the peduncle, trifid, and each branch much longer than the space below it, to the origin of the peduncle, supporting three cordate, acuminate leafets, of which the lateral ones are oblique, the central equal, glabrous above, hairy below, 3–5-nerved, reticulate. *Peduncle* of unequal lengths, taking its origin (about 1½ inch) below the subdivision of the petiole, and sometimes as long as the leaf, generally shorter, round, and glabrous. *Flowers* violet, large and handsome, subtended by 3–4 deciduous unequal *bracteæ*, racemose in the upper half of the peduncle, cernuous; the lower pedicels occasionally supporting two flowers, the others one only. *Sepals* four, ovato-lanceolate, acute, spreading wide, undulate. *Petals* imbricated, rounded, cucullate, inflected in the edge, each with a subulate ascending spur, rather longer than the petal. *Stamens* erect, shorter than the limb of the petals; anther-cells opening by valves rolling upwards rather on the outside, so as to bear a flattened undivided surface next the pistil; pollen yellow, granules small, oval; filaments short, colourless. *Pistil* rather longer than the stamens, but shorter than the limb of the petals; germen green, oblong; style shorter than the germen, somewhat thickened upwards, cupped at the apex, and bearing the stigmatic surface on the edges of the cup. *Ovules* about ten, attached to a lateral placenta.

This, the handsomest species of the genus, flowered freely under a handglass in the open border, at the nursery garden of Mr Cunningham, Comelybank, Edinburgh, in April and May 1839.

Fabiana imbricata.

F. *imbricata*; fruticosa, ramosissima; foliis imbricatis, squamæformibus, floribus solitariis, terminalibus, lilacinis.—*Spr*.

Fabiana imbricata, *R. & P.* Flor. Peruv. 2. 12. t. 122.—*Pers*. Synops. 1. 176.—*Spr*. Syst. Veget. 1. 615.

Description.—*Shrub* (in the specimen described 1 foot high) erect, much branched, branches spreading, shortly glanduloso-tomentous, glutinous, green. *Leaves* small, crowded, fleshy, subulate, recurved, gibbous on the outside at the base, glandular over their whole surface. *Flowers* (1½ inch long) white, solitary, on short terminal peduncles, slightly glutinous. *Calyx* green, slightly irregular, 5-toothed, glabrous, wrinkled. *Corolla* hypogynous, tubular, glabrous within and without; tube clavato-cylindrical at the base; limb short, plicate, after expansion revolute. *Stamens* 5, perigynous, free above the cylindrical portion of the corolla, unequal, the longest projecting into the throat, filaments glabrous; anthers short, oblong, yellow, versatile, fixed by the back, lobes bursting laterally; pollen granules minute. *Pistil* scarcely exceeding the longest stamens; stigma green, capitate, cleft across the vertex; style filiform, slightly swollen towards the stigma; germen placed upon a 2-lobed, deep orange-coloured disk, green, ovate, slightly compressed, bilocular, ovules very numerous, attached to and covering the dissepiment.

This is an exceedingly pretty plant, flowering profusely in the greenhouse, bearing forcing in stove heat, and lasting in beauty a long while. It came into blossom in one of the stoves in Mr Cunningham's nursery garden at Comelybank, Edinburgh, in the end of March, and continued covered with flowers for more than three weeks. In the greenhouse of the same extensive establishment, the blossoms are not yet (18th April) expanded. Mr Cunningham received the plant from France, and understood that it had been imported from Chile.

The flowers of *Fabiana imbricata* are said to be lilac, and as they are in the specimen described pure white, there may be a doubt about the specific name; but I cannot see any distinction between this plant and a specimen which I have from Dr Gillies as *F. imbricata*, and the description applies in every thing but the colour of the flower. I do not see any sufficient specific distinction between our plant and the specimens from Conception distributed by Mr Cumming with the No. 140, though this seems more robust, and to have leaves proportionally shorter and more adpressed.

Heterotropa.

Generic Character.—Perianthium ventricosum coloratum trilobatum, fauce plicata introflexa, introrsum reticulato-venosum, venis verticalibus prominentibus. Stamina 12; 6 exteriora stigmatibus opposita, filamentis triangularibus adnatis: antheræ basifixæ subintrorsæ; 6 alterna sessilia: antheræ evidenter extrorsæ. Styli 6 concreti, stellatim expansi, singulis obcordatis, parte inferiore stigmatifera. Stigmata ovato-attenuata papillosa. Ovarium 6-loculare, liberum.—*Morren et Decaisne*, Ann. Sc. Nat. 2de serie,—Botanique, tome ii. p. 314, t. 10.

Heterotropa asaroides, *Morren et Decaisne*, l. c.

Description.—*Stem* very short. *Stipules* large, cordato-ovate, concave above, keeled below, coloured, ciliated, otherwise subglabrous. *Leaves* petiolate, deeply cordate, blunt, slightly waved and reflected in the edge (from the blunt auricles to the apex about 4–5 inches long, 3¼ inches broad), dull green, and irregularly blotched with lighter coloured spots on the upper surface, where also it is slightly pubescent, especially towards the edges, glabrous, shining, and having purple veins, and occasionally purple blotches below; petiole (3¼ in. long) coloured, glabrous, deeply channelled above, rounded below. *Flower* perfumed, somewhat like a ripe apple, sessile, as large as a walnut, cartilaginous; tube spheroid, glabrous on the outside, of dull purple colour, blotched with grey; limb of darker purple, 3-fid, reflected, segments large, rounded, undulate, above transversely wrinkled, hairy, becoming glabrous towards the throat, where the wrinkles are drawn up into thin edges of brighter purple, waved, and the outer ones crested with white; throat half closed by a transverse plate of the same structure, leaving a triangular opening in the middle; inner surface ribbed, with honey-combed reticulations between the ribs. *Stamens* 12, filaments coloured, very short, in six reduced almost to a lanceolate connective projecting beyond the anther, which consists of two distinct cells placed on the outside, in the other six, which alternate with these, the connective is narrower, more blunt, and the anther-cells sublateral. *Pollen* white, granules minute, spherical. *Style* short, fleshy, deep purple, with six vertical plates or angles opposite to the stamens with sublateral cells, and six terminal nearly horizontal obcordate lobes of the same colour and texture as the rest of the style, covering the stamens, each lobe attached along the middle of its lower surface to the top of one of the vertical plates, and each having, as if pendent from its sinus, an erect ovato-acuminate stigma, these alternating with cucullate spaces formed by the lower sides of the adjoining obcordate lobes of the style. *Germen* half inferior, 6-locular, the loculaments being placed immediately under the stigmatic surfaces, the dissepiments under the cucullate spaces. *Ovules* 10, in two rows in each loculament, horizontal, placentæ central.

This most curious plant was imported into Europe from Japan by M. Von Siebold. We received it at the Botanic Garden, Edinburgh, from Mr Young at Epsom, in 1838. It flowered in the greenhouse in the end of February 1839, and remained long expanded. Soon after this it flowered more freely under a hand-glass in the open border at the nursery ground of Mr Cunningham, Comelybank, and the last blossom did not decay till after the beginning of June.

Pimelia decussata, var. diosmæfolia.

P. *decussata;* involucro tetraphyllo, foliis rameis dissimile, foliolis late ovatis, semicoloratis, utrinque glabris; capitulo terminale; perianthii tubo hispido; foliis oppositis, decussatis, ovalibus, coriaceis.—*Br.*

Var. *diosmæfolia;* foliolis involucri ovatis, acutis, lanato ciliatis, capitulum subæquantibus; perianthiis saturate roseis.

Pimelia diosmæfolia, *Hortul.*

Description of the variety.—*Shrub* erect. *Branches* lax, spreading. *Leaves* oblong, coriaceous, opposite, decussating, reflexed, mucronulate, dark green above, glaucous below. *Capitulum* crowded, terminal, rather small. *Involucre* of 4 ovate acute subcoloured leaves, which are glabrous on both sides, but ciliated on the edges, scarcely shorter than the capitulum. *Perianth* bright rose-coloured, hispid on the outside, hairs very long in the lower half of the tube, and on the outside of the limb, shorter and subappressed on the upper half of the tube. *Stamens* inserted into the throat of the perianth, scarcely half the length of the limb. *Style* much exserted, lateral. *Stigma* capitate. *Germen* oblong, glabrous, green.

This very pretty plant was received at the Botanic Garden, Edinburgh, from Mr Makoy of Liege, under the name of *Pimelia diosmæfolia*, but I am convinced it is only a variety of *P. decussata*, distinguished by its darker rose-coloured flowers, its more ciliated involucrum, and its dense, small, more crowded capitula. It very nearly resembles *Pimelia Hendersonii*, distinguishable chiefly by its smaller capitula, and by its less acute foliage, and, I fear, is a link by which this will also be united to *P. decussata*, which is known to be a variable species. The form now described is very handsome, very worthy of cultivation, and readily distinguished. It should receive the ordinary treatment and protection of the other species of the genus, and will be found to flower very abundantly.

Solanum fragrans.

S. *fragrans;* arborescens; foliis geminis, ovatis cordatisve, inæqualibus, integerrimis; racemis solitariis ex axillis ramorum; floribus secundis, pedicellis inferioribus maxime elongatis; calyce 5-fido; corollis lato campanulatis, profunde 5-partitis, limbo reflexo; staminibus geniculatis, stigmate dilatato concavo.—*Hooker.*

Solanum fragrans, *Hooker*, in Bot. Mag. 3684.

Description.—Whole plant unarmed. *Stem* erect, shrubby (12 or 13 feet high in the specimen described by Hooker, in ours, a cutting from his, flowering freely though less than 3 feet high), glabrous, pubescent. *Branches* spreading, green. *Leaves* petiolate, alternate and distant below, subopposite above, unequal, and approaching each other on one side of the branches, coriaceous, ovato-cordate, shortly acuminate, obscurely sinuated, and slightly waved, slightly reflexed in the edges, distantly denticulate, bright green above and glabrous, paler below and somewhat roughened; middle rib strong, veins few, conspicuous; petiole about half the length of the lamina. *Racemes* solitary, in the axils of the branches, pendulous, many-flowered, ebracteate; pedicels single-flowered, articulated near the base, becoming elongated and reflexed, secund. *Calyx* 5-cleft, yellowish-green, as well as the peduncles and pedicels the petioles and young shoots slightly glanduloso-pubescent, segments entire. *Corolla* coriaceous, deciduous, campanulate, 5-cleft, at last parted to the apex of the calyx segments and spreading wide, glabrous and shining, at first purple, brighter and more lilac within, becoming much paler, rather more red, and at last yellowish, segments acute, alternate with those of the calyx, valvate, reflected. *Stamens* 5, alternate with the segments of the corolla, inserted into its base, and shorter than it; filaments stout, short, spreading; anthers connivent at the apex, purple, ovate, opening by two pores under the stigma, where they are in contact with the top of the style; connective large, yellow, gibbous on the outside at the base of the anther; pollen white, granules small, round. *Pistil* glabrous, green, rather longer than the stamens, about half the length of the corolla; stigma dark green, dilated, forming a shallow oval fleshy cup, with two large swellings within; style stout and clavate; germen oval, as well as the style yellowish-green, bilocular; placentæ entire, large and 2-fid, covered with numerous ovula.

This plant was propagated from a cutting of the Glasgow plant, and flowered freely in June 1839 in a pot kept in stove heat. I did not at any time perceive the perfume mentioned by Sir William Hooker, and from which he has derived the specific name. It is therefore, as in many other examples of vegetable perfumes, perceptible only at particular hours, or in particular states of the weather.

Thysanotus proliferus.

T. *proliferus*; foliis linearibus, longissimis, canaliculatis, glabris; scapo prolifero; umbellis multifloris; pedicellorum articulo inferiore bracteis longiore (exterioribus exceptis); staminibus tribus, styloque decurvis. —*Lindl.*

Thysanotus proliferus, *Lindl.* in Bot. Reg. 8. ann. 1838.

Description.—*Root* fibrous. *Stem* erect, compressed, glabrous, green, branched, leafless, excepting at the base, or at its subdivision when this is low, where occasionally a single short leaf or greatly enlarged bractea is produced. *Leaves* linear, numerous at the base of the stem, dark green or subglaucous, glabrous, channelled, equal to the stem, but, from decaying at the apex in the specimen described appearing shorter. *Umbels* many-flowered, one or two upon each stem, approximated or remote, in the former case the lateral one sessile, in the latter supported upon a stalk more or less elongated, at the origin of which there are generally two flowers. *Bracteæ* ovato-subulate, herbaceous, with scariose edges. *Pedicels* articulated above the base, each springing from the axil of a bractea, the lower joint being longer than this in all the flowers excepting a few on the outside; upper joint longer than the lower, and more glaucous. *Sepals* ovato-oblong, spreading wide, mucronate, on the outside glaucous, within purple, edges scariose. *Corolla* (1¼ inch across) very handsome, of bright lilac colour; petals oblong, in the centre having an opaque linear stripe, which is glaucous and 3-ribbed externally, internally of deep lilac, sides of clear and paler lilac, and edges with long silky fringes of the same colour. *Stamens* 3, bent to one side of the flower; filaments short and colourless; anthers more than twice as long as the filaments, curved, having two slightly divaricated yellow lobes at the base, above of a deep purple colour; pollen white, granules very minute. *Pistil* rather longer than the stamens, curved in the opposite direction from them; stigma minute, terminal; style subulate, grooved on three sides; germen ovate, grooved; ovules 2 in each loculament.

This very beautiful species flowered at the garden of the Caledonian Horticultural Society in May 1839, a plant having been obtained from Messrs Veitch and Son, nurserymen, Exeter, in the autumn previous. It has grown in a mixture of peat and leaf-mould, and been kept in stove heat, having been found elsewhere too tender for the greenhouse. Its long succession of flowers will no doubt be developed during the whole summer, each flower expanding betwixt 8 and 9 in the morning, and in clear weather closing permanently about 2 P. M.

Proceedings of the Royal Society of Edinburgh.

1839, *January* 21.—Lord GREENOCK Vice-President, in the Chair. The following communications were read :—

1. On the Colour of Steam under certain circumstances. By Professor Forbes.

The author accidentally remarked, that the colour of the Sun seen through vapour issuing from the safety-valve of a locomotive engine is deep red, exactly similar to that which a column of smoke or a smoked glass gives to it.

He next noticed that this colorific character of steam extended but a short way beyond the orifice, and that it gradually became more opake, and perfectly white like noon-day clouds, both for transmitted and reflected light. At moderate thicknesses, in this state, its opacity is complete.

These observations were fully confirmed by direct experiments on high-pressure steam, made at Glasgow in December last. At the moment of issuing from the steam-cock, it is perfectly transparent and colourless; at some distance from the orifice it becomes transparent and orange-red; but, still farther off, it is white, and merely translucent. These properties were traced in steam from a pressure above that of the atmosphere of 55 lb., down to an excess of only three or four; and as in all cases the redness of the transmitted light was more or less distinctly seen, (and an excess of 10 or 15 lb. does as well as any higher pressure), it was concluded that the effect of partial condensation in producing the phenomenon, would be rendered visible in great thicknesses of vapour of the lowest tension.

The great analogy of the colour of steam to that which the clouds assume at sunset, or distant lights in certain conditions of the atmosphere, lead the author to suggest this singular property of condensing vapour as the probable cause of those phenomena, of which no satisfactory explanation could be given whilst this fact remained unknown. The prognostics of weather derived from the colours of the sky also receive elucidation from the fact.

Judging from the similarity of colour of steam and that of nitrous acid gas, and the remarkable power of absorbing certain definite rays of the spectrum discovered in that gas by Sir David Brewster, the author thought it probable that similar lines might be discovered in the spectrum formed by light transmitted through steam, and that these might be found to coincide with the *atmospheric lines* of the spectrum noticed by the same philosopher. The experiment was made with great care, but the expected result has not been hitherto obtained. The general action of steam on the spectrum is to absorb the violet, blue, and yellow rays, finally leaving only the red and orange, with an imperfect green.

Since a portion of watery vapour in a confined space, originally transparent and colourless, may become, by mere change of temperature, first deep orange-red and transparent, and finally white and semiopake, the author notices another analogy with the singular effect of temperature in deepening the colour of nitrous acid gas, and thinks that these facts may one day throw some farther light on the difficult subject of the mechanical constitution of vapours, and particularly of clouds.

2. On the meaning of the Homeric terms *θεμισ, δικη, τιμη, ποινη*, with their more important compounds and derivatives.

By the Venerable Archdeacon Williams.

February 4.—Dr Abercrombie, V. P., in the Chair. The following communication was read :—

The Colours of the Atmosphere considered with reference to a paper " On the Colour of Steam under certain circumstances," read at the last meeting of the Royal Society.

By Professor Forbes.

The object of this paper was to develope an application of the fact communicated by the author on the 21st January. It was then remarked, that the discovery that steam in a certain stage of condensation is deeply red-coloured for transmitted light, seemed to offer a probable solution of a difficulty which has never yet been fairly met, namely, the red colour of clouds at sunset, and the redness of light transmitted through certain kinds of fogs.

A pretty full history of theories proposed to account for the colours of the atmosphere was first given; it was obtained in almost every case from an examination of the original authorities. These theories were reduced under three general heads, exclusively of that of Göthe, and of most writers before Newton, that the blue colour of the sky results from a mixture of light and shade; and that of Muncke, that that colour is merely *subjective*, or arises from an ocular deception. The remaining theories are:

(1.) That the colour of the sky is that transmitted by pure air, and that all the tints it displays are modifications of the reflected and transmitted colours. This is more or less completely the opinion of Mariotte, Bouguer, Euler, Leslie, and Brandes.

(2.) That the colours of the sky are explicable by floating vapours acting as thin plates do, in reflecting and transmitting complementary colours. This is the theory of Newton and most of his immediate followers, and more lately of Nobili.

(3.) On the principle of opalescence and of specific absorption, depending on the nature and unknown constitution of floating particles. This head is intended to embrace the various opinions of Melvill, Delaval, Count Maistre, and Sir D. Brewster.

To the last named philosopher, however, the merit is due of having conspicuously turned attention to the important, complex, and hitherto unexplained phenomena of absorption, which he has proved to be totally inconsistent with Newton's theory of the colours of Nature, (considered as those of thin plates); and he has farther demonstrated the inapplicability of it in the case of the colours of the atmosphere, by shewing that their constitution is wholly distinct from that which any modification of Newton's theory would assign, by a series of experiments of which as yet the results only are announced.

Since, then, the constitution of the atmospheric colours analysed by the prism resembles that produced by absorption, the question is, To what medium are we to refer that absorptive action? Evidently not to pure air, since a distant light is red in a fog, and in clear weather white, or nearly so. The author is disposed to attribute the effect to the presence of vapour in the very act of condensation. This intermediate or colorific stage occurs between the colourless and transparent form of steam wholly uncondensed, and that which may be termed the state of *proximate* condensation in which it is seen to issue from the spout of a tea-kettle, when it is likewise colourless, but semiopake. During the transition, it was shewn in the former paper that steam becomes intensely red, and remains transparent. The absorptive action resembles then, so far, that of the atmosphere observed under certain meteorological conditions; the dark lines and bands noticed by Sir David Brewster in the atmospheric spectra have not been discovered, and so far the analogy is as yet imperfect.*

In applying this theory to the colours of sunset in particular, the author quotes many acknowledged facts to prove that the redness of the sky is developed precisely in proportion to the probable existence of vapour in that critical stage of condensation which should render it colorific. And he applies the same reasoning to account for the prognostics of weather, drawn from the redness of the evening and morning sky.

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* Some plausible reasons are assigned why these bands should not have appeared in the experiment as it was made, when steam in every stage of condensation must necessarily have been present; nor does it seem easy to devise a form of experiment free from this objection. A very important observation would be to examine the spectrum produced by a distant artificial light seen through a red fog.

February 18.—Dr ABERCROMBIE, Vice-President, in the Chair. The following communications were read:—

1. Notice of some observations made during the Storm of January 1839. By John Scott Russell, Esq.
2. On Fresnel's Law for the Intensity of Reflected and Refracted Light. By Professor Kelland.

March 4.—Dr HOPE, Vice-President, in the Chair. The following communications were read:—

1. On a new Galvanic Battery, and an improved Voltameter. By Martyn Roberts, Esq. Communicated by Sir John Robison.
2. Notice upon the Alcoholic Strength of Wines. By Dr Christison.

Various accounts have been given of the alcoholic strength of wines by Mr Brande, Julia-Fontenelle, and others. The author has been engaged for some time in experiments for determining the proportion of alcohol contained in various wines of commerce, and also the circumstances which occasion a variety in this respect. The present paper is an interim notice of the results.

The method of analysis consisted in the mode by distillation, which was applied with such contrivances for accuracy that nearly the whole spirit and water were distilled over without a trace of empyreuma, and without the loss of more than between 2 and 6 grains in 2000. From the quantity and density of the spirit, the *weight* of absolute alcohol of the density 793.9, as well as the *volume* of proof spirit of the density 920, was calculated from the tables of Richter founded on those of Gilpin.

The author has been led to the general conclusion that the alcoholic strength of many wines has been overrated by some experimentalists, and gives the following table as the result of the investigations he has hitherto conducted. The first column gives the per-centage of absolute alcohol by weight in the wine, the second the per-centage of proof spirit by volume.

	Alc. p. c. by weight.	P. Sp. p. c. by volume.
Port—Weakest,	14.97	30.56
Mean of 7 wines,	16.20	33.91
Strongest,	17.10	37.27

	Alc. p. c. by weight.	P. Sp. p. c. by volume.
White Port,	14.97	31.31
Sherry—Weakest,	13.98	30.84
Mean of 13 wines, excluding those very long kept in cask,	15.37	33.59
Sherry—Strongest,	16.17	35.12
Mean of 9 wines very long kept in cask in the East Indies,	14.72	32.30
Madre da Xeres,	16.90	37.06
Madeira, all long in cask in East Indies — Strongest	14.09	30.80
Weakest	16.90	36.81
Teneriffe, long in cask at Calcutta,	13.84	30.21
Cercial,	15.45	33.65
Dry Lisbon,	16.14	34.71
Shiraz,	12.95	28.30
Amontillado,	12.63	27.60
Claret, a first growth of 1811,	7.72	16.95
Chateau-Latour, first growth 1825,	7.78	17.06
Rosan, second growth 1825,	7.61	16.74
Ordinary Claret, a superior " vin ordinaire,"	8.99	18.96
Rives Altes,	9.31	22.35
Malmsey,	12.86	28.37
Rudesheimer, superior quality,	8.40	18.44
Rudesheimer, inferior quality,	6.90	15.19
Hambacher, superior quality,	7.35	16.15
Giles' Edinburgh Ale, before bottling,	5.70	12.60
The same Ale, two years in bottle,	6.06	13.40
Superior London Porter, four months bottled,	5.36	11.91

In addition to certain obvious general conclusions which may be drawn from this table, the author stated, as the result of his experiments, that the alcoholic strength of various samples of the same kind bears no relation whatever to their commercial value, and is often very different from what would be indicated by the taste even of an experienced wine-taster.

Some observations were next made on the effect produced on the alcoholic strength of wines by certain modes of keeping or ripening them, more especially by the method employed in the case of sherry, madeira, and such other wines, which consists of slow evaporation for a series of years through the cask, above all, in hot climates. The researches made by the author on this head are not yet complete; but he is inclined to infer, from the experiments already made, that, for a moderate term of years, the proportion of alcohol increases in the wine, but afterwards, on the contrary, diminishes; and that the period when the wine begins to lose in alcoholic strength is probably that at which it ceases to improve in flavour. The increase which takes place at first in the alcohol of wine undergoing evaporation through the cask, appeared at first view parallel to the fact generally admitted on the authority of Söemering, that spirit becomes stronger when confined in bladder, or in a vessel covered with bladder, in consequence of the water passing out by elective exosmose.

The author, however, on repeating the experiments of Söemering, as related by various writers (for he could not obtain access to the original account of them), was unable, by any variation of the process he could devise, to obtain the results indicated by the German anatomist. Constantly the spirit, whatsoever its strength, whether proof spirit or rectified spirit, became weaker. It was observed at the same time, that, if the bladder containing spirit was enclosed in a confined space with quicklime, the spirit slowly became absolute alcohol of the density 796, in consequence of a permanent atmosphere of alcohol being speedily formed, while the watery atmosphere was absorbed by the quicklime as fast as it was produced. Subsequently it was proved that the bladder was not essential to the process; for an open cup of rectified spirit, inclosed in a confined space with quicklime, to absorb the water which arose from the spirit, became in two months absolute alcohol of the density 796. Professor Graham of London some time ago proved the analogous fact, that spirit might be thus rendered pure alcohol in the air-pump vacuum. A vacuum, however, is, upon principle, as well as in fact, not necessary for the process; it merely accelerates it. The new method is obviously applicable on the great scale for obtaining absolute alcohol, wherever time may be allowed.

March 18.—Dr ABERCROMBIE, Vice-President, in the Chair.

The following communications were read :—

1. Notice respecting the Drying-up of the Rivers Teviot, Clyde, and Nith, and their tributaries, on the 27th November 1838. By David Milne, Esq.

The phenomenon was in the first instance described, and certain views were afterwards offered explanatory of it.

It appears that, betwixt 10 P. M. on the 26th November, and 6 A. M. on the 27th November, the channels of the Teviot, Clyde, and Nith, became nearly dry for a great part of their course, so that scarcely any current flowed in them. All the mills on the Clyde, as far down as several miles below New Lanark, were stopped from want of water. The Nith was nearly dry as far down as Enterkinefoot; and the mills on it, and on its tributaries, were stopped. This was the case also on the Teviot. The phenomenon was most strikingly manifested in the higher parts of the rivers, near their sources. The small streams from which they derive their supplies, were in general completely dried up. The rivers, in the lower parts of their course, were not entirely deprived of their current; nor were the rivulets, which there supplied them, nearly so much affected as the rivulets in more elevated districts.

The desiccation continued all the morning, forenoon, and part of the afternoon of the 27th November. When the current was restored, it returned not with a sudden rush, but gradually; nor when the current was restored, did the waters rise much above their ordinary level.

With reference to the cause of the phenomenon, it was stated, that various explanations had been suggested. Some persons had attributed it to the *high wind* obstructing the flowing of the current; others, to the *frost* in forming barriers of ice on the caulds or dam-heads; others, again, had suggested that the phenomenon might be connected with an earthquake. In support of this last theory, it was mentioned, that Professor Phillips had, in a recent work on geology, attributed to this cause the drying-up of the English rivers Trent and Medway, in the 12th century.

Mr Milne stated that he adopted none of these views, and that he thought the phenomenon might be accounted for by the united action of the frost and wind which prevailed during the night of the 26th November. After four o'clock that afternoon, the thermometer all over the south of Scotland sunk to 26°, at which point it remained for several hours. Accompanying this frost, there was a gale of wind from the east, which had the effect of very rapidly reducing the temperature of exposed and unsheltered spots. In this way, the small and shallow streams flowing in open drains and rivulets, or oozing through mosses and marshes in the hills, were soon frozen and arrested. But, on the other hand, larger bodies of water flowing rapidly in the main channels, at a lower level, and sheltered by high or wooded banks, could not in the same space of time lose enough of their temperature to be frozen. The waters thus ran off, without the usual renewal of supplies from the sources, so that the channel or bed of the river became speedily drained.

The reason of this phenomenon not happening more frequently appears to be, that there is very seldom a gale of wind in this country accompanied by a severe frost; and even on this occasion, the frost was not equally intense over the whole island. When a severe frost sets in, there is usually but little wind, so that the water in the *upper* parts of the river, is not liable to be cooled more rapidly than in the lower and more sheltered parts of its course. Though the sources will, in that case, to a certain degree, be frozen, and so, part of the usual supply cut off, the main body of the stream is frozen likewise, whereby the velocity of its current is diminished, by the obstruction of the ice at the bottom and at the surface of the current. So that if only half the usual supply is furnished to the river from its partially frozen sources, there will be no diminution in the quantity of water flowing in the main bed of the river, if it flows off with half its usual rapidity. This is the ordinary way in which frost acts on the rivers, in this country. But when, as on the night of the 26th November, the frost is accompanied with a strong and keen wind, which lasts for only a few hours, it freezes the water in the small rivulets near the sources of the rivers in high and exposed situations, whilst it has not time to freeze even the surface of the deeper and more rapid currents flowing in the lower parts of the rivers.

The easterly gale which, by its low temperature, produced this phenomenon, continued to blow until about 7 or 8 A. M. on the morning of the 27th November. The temperature of the atmosphere then underwent a sudden change, as indicated both by the barometer and the thermometer. This change was brought about by the advent of two storms, which came from southern latitudes, and one or perhaps both of which had, on the morning of the 27th, begun to affect the upper regions of the atmosphere, and load them with warm vapour.

2. Memorandum on the Intensity of Reflected Light and Heat. By Professor Forbes.

April 1.—Dr Hope, Vice-President, in the Chair. The following communications were read:—

1. On the Theory of the Motion of Waves. By Professor Kelland.

2. On a New Method of shewing the Unequal Expansion of Crystals. By Professor Forbes.

April 15.—Lord Greenock in the Chair. The following communications were read:—

1. Notice respecting the relative Voltaic agency of Circuits of Copper and Zinc, and Zinc and Iron. By Martyn Roberts, Esq. Communicated by the Secretary.

2. Investigation of analogous properties of Co-ordinates of Elliptic and Hyperbolic Sectors. By W. Wallace, LL.D., Emeritus Professor of Mathematics, University of Edinburgh.

3 On the Newer Tertiary or Pliocene Deposites of Scotland. By James Smith, Esq. of Jordanhill.

4. On certain circumstances affecting the Colour of Blood during Coagulation. By Dr P. K. Newbigging. Communicated by Professor Forbes.

The author described in this paper certain anomalous appearances presented by venous blood when left in contact with coloured porcelain. When blood drawn from a vein is either allowed to coagulate in a porcelain cup, or after coagulation is left in it for some hours, the dark purple tint characteristic of venous blood is found to be altered to the bright arterial hue, wherever it was in contact with any elevated device of the green colour, which is communicated by means of protoxide of chrome. In one instance the same effect was produced by a device of a crimson tint; but in more than sixty trials with this and every other variety of colour used in ornamenting porcelain, the author could observe no such effect as was invariably produced by patterns of a green tint. The effect was scarcely apparent, if the pattern was not somewhat elevated. Mere elevation of the porcelain, however, is not the cause of the change of colour. Adhesion of a little oxygen of the air to the surface of the pattern is not its cause; for no change is produced by the green devices of porcelain on a gelatinous mass of gelatin and protoxide of iron, which is quickly rendered brick-red wherever it is put in contact with oxygen. Neither is the change owing to any peculiar arrangement of the molecules of the blood, because it is produced equally on coagulated blood and upon that which is allowed to coagulate in the cup. The author is compelled, therefore, simply to record the fact, that objects on porcelain of a green colour produce their impression, and this with extreme accuracy, on venous blood left in contact with them, and that the change of colour which takes place seems identical with the florid hue occasioned by arterialization of the blood from ordinary causes.

5. On two Storms which passed over the British Islands in the end of November 1838. By David Milne, Esq. Advocate.

It was stated, that the first indication of the advent of these two storms to the British islands was given by the barometer. On the 25th and 26th November, the barometer in all parts of the United Kingdom began to sink; and it is important to observe, that, not only when this sinking began, but likewise for nearly two days after, the wind was blowing E. or NE., and was accompanied by a severe frost, circumstances which of themselves cause the mercurial column to rise.

The exact hours on the 25th and 26th November at which, in different places, the barometer began to sink were noticed, from which it appeared that the sinking took place in the south part of the United Kingdom about five or six A. M. on the 25th, and in the north of Scotland about two or three in the morning of the 26th.

I. Now, as the first storm did not reach England, or, in other words, that part of the surface of the globe, till about noon on the 26th, and the north part of Scotland till the night of the 27th or morning of the 28th, it is obvious that the upper regions of the atmosphere were, in the British islands, affected before the lower regions by a period of about thirty hours. It follows, that the *upper* part of the storm preceded the *lower* part, in its progress over the surface of the globe.

This first storm commenced at Cork on the 26th, at 11 A. M.; at Cornwall, about noon; at Dublin, about 3½ P. M.; at the Isle of Man, early in the morning of the 27th; at Lismore, on the night of the 27th; and Cape Wrath, not till the 28th November.

The wind at all these places, on the commencement of the storm, blew from SE. or SSE.; it afterwards veered to SW., and on the cessation of the storm it was blowing from NW. The veering to SW. took place in Cornwall during the *forenoon* of the 27th; it took place in Dublin in the *evening* of that day.

Heavy gusts came from a point due west, or rather to the north of west, shortly before the cessation of the storm. These were felt in Cornwall in the *afternoon* of the *27th;* they did not commence at Pladda (off the coast of Ayrshire) till the *night* of the 28*th* November.

From these and other similar data, it was concluded, that the storm moved progressively in a N.NE. direction, at the rate of ten or eleven miles an hour.

The progress of this storm from southern latitudes was next described, by reference to various places both on sea and land, as far south as Gibraltar, at all of which it had been severely felt. On the 22d and 23d, a storm from the S. was experienced at the mouth of the Garonne. Off the NW. coast of Portugal, three vessels were dismasted by a hurricane on the 22d; at Gibraltar, there was a storm on the 21st November. It was most probably one and the same storm which passed over all these places, beginning at Gibraltar on the 21st, and reaching the British islands on the 26th November;—seeing it would arrive there at the very time that the first storm begun in England, and had the same direction and rate of movement.

Some circumstances were stated, shewing that this storm had probably a rotatory as well as a progressive motion. These were, (1.) the great velocity of the wind in the storm, compared with the actual motion of the storm; (2.) the veering of the wind in the storm from SE. to NW.; (3.) the greater violence of the gusts from S. and SW.,—the progressive and rotatory motions then coincided.

II. The second storm begun on the SW. coast of Ireland about 2 A. M. on the 28th; at Cork, about 3 or 4 A. M.; Cornwall, about 5 A. M.; Plymouth, about 9 A. M.; and Fairnborough (near Bagshot), about 10 A. M.

At all these places there was, during the previous night, a calm, or light airs from the westward.

This storm reached Dublin about 1 P. M. on the 28th; Glasgow, about 3 P. M; and Kirkaldy, between 4 and 6 P. M. It travelled northward, therefore, at the rate of about twenty miles an hour.

This inference was corroborated by the periods at which, in different places, the wind in the storm veered from SE. to SW. This veering, which took place in Cornwall about *noon* on 28*th*, did not take place at Kirkaldy till the *afternoon* of the 29*th*.

A separate and still more striking confirmation is afforded by the period when the barometer reached its lowest point at different places. Its greatest depression in the Bristol Channel occurred at noon on the 28th; in Edinburgh and Glasgow, at 12 h. 16 m. on the 29th; in Kinfauns Castle (Perthshire), at 8½ P. M. on the 29th.

That this storm had, like the previous one, a progressive motion to N.NE., there can be no doubt; and from its moving in nearly the same track, but travelling with twice the rapidity, it overtook the first storm about the middle of Scotland; in consequence of which, the indications of two separate storms became indistinct in these northern parts.

There were many circumstances shewing that this second storm had a rotatory as well as a progressive motion, and that the centre of the stormy circle travelled along a path considerably to the west of the British islands. (1.) One of these was the greater magnitude of the angle made by the wind in veering, and the greater rapidity with which the veering was effected, in places situated to the west, than in places situated to the east. At Limerick, the wind veered 158° in 28½ hours; at Penzance, 112° in 24 hours; at Greenwich, 79° in 24 hours; at Kinfauns, 90° in 24 hours. (2.) When the storm ceased, the wind was blowing more from the north in places situated to the west. On the west coast of Ireland, it was blowing W. or W.NW.; in the south-west of Scotland, and at Holyhead, it was blowing W.SW. or SW. (3.) The barometer was lowest towards the west; and by classing together those places where the depression was the same, it appeared that these places lay in lines or zones running in a N.NE. direction, in each of which the barometer stood at a lower level in proportion as its situation was toward the west. In London and Greenwich, the barometer stood at its greatest depression at 28.70; at Limerick, it stood at its greatest depression at 27.49, both observations being reduced for height. (4.) It was found that a storm had traversed the eastern part of the Atlantic, moving in a N.NE. direction, having been felt at the Garonne on the 27th, 28th, and 29th; at Oporto, from the 24th to the 27th; at Lisbon, on the night of the 23d. It appeared that several vessels in the middle of the Atlantic had, on the 28th, been dismasted by a NW. gale, whilst the Great Western steamer, some degrees to the NW. of these ships, though on that day and the next she experienced no gale, encountered a heavy swell from the N.NE.

It appeared from these, and some other data which were detailed, that this storm moved N.NE. over the Atlantic, at the rate of about twenty miles an hour; that it had also a rotatory motion, and that the centre of the circle passed very considerably to the west of the British islands, so that it was only a segment of the storm which swept over these islands.

It was mentioned that, before the most furious part of the gale reached England, and before the barometer reached its greatest depression, a storm-wave had entered the Irish and Bristol Channels, and caused, in most of the harbours there, on the night of the 28th November, an unusually high tide.

Subjects proposed for Honorary Premiums by the Wernerian Natural History Society.

EDINBURGH, 20*th April* 1839.

The Wernerian Natural History Society offer the following Honorary Premiums; open unconditionally to all scientific naturalists. It is understood that the successful essays, and such drawings and specimens as accompany them, become the property of the Society: Further, that in the event of the Society not publishing the essays, the authors may be allowed to publish them on their own account.

HYDROGRAPHY.

1. TEN SOVEREIGNS, or a suitable piece of plate of that value, for an approved Essay on the Temperature, Magnitude, Chemical Composition, and Geological relations of the *Springs* of Scotland. This Essay must be founded on the actual observations and experiments of the Author.—Time, February 1844.

2. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Temperature, Colour, Chemical Composition, Mechanical admixture, Magnitude, Velocity, and Alluvial formations of any one of the following *Rivers* in Scotland, viz. the Tweed, Tay, Dee in Aberdeenshire, or Spey.—Time, February 1841.

GEOLOGY

3. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Erratic Blocks or Boulders of Scotland and its Islands.

N. B. The Author will be expected to treat of the Mineralogical and Palæontological Characters, the Physical and Geographical Distribution of these Boulders, and, from the data thus afforded, endeavour to account for their origin and distribution. A Map or Maps illustrative of their distribution will also be required.—Time, February 1841.

4. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Experimental and Practical Essay on the Mineralogical Constitution, and Chemical Composition of the Trap-Rocks of Scotland. A collection of specimens required.—Time, February 1842.

5. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Experimental Essay on the Chemical Composition of Al-

tered or Metamorphic Rocks met with in Granite, Porphyry, Serpentine, and Trap districts.

N.B. It is expected that the Author will, where necessary, examine chemically the Unaltered Rocks associated with the Altered. Collection of Specimens required.—Time, February 1842.

6. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Fossil Organic Remains found in the transition-rocks and the carboniferous systems of Scotland. The new or unfigured Species to be accompanied by Drawings. Collection of Specimens required.—Time, February 1844.

7. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the *Raised Sea-Beaches* met with in Scotland and its islands. Specimens of the Shells, &c. required.

N.B. Mr SMITH's paper, in the 8th Volume of the Society's Memoirs, recommended to the notice of those who may take up the subject.—Time, February 1844.

ZOOLOGY.

8. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Entomology of the Three Lothians, and the River District of the Forth, with a Collection of Specimens.—Time, February 1844.

9. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Series of Drawings and Descriptions of the Microscopic Animals inhabiting the waters of any of the following arms of the sea and lakes, viz. Firth of Forth, Firth of Clyde or Loch Fyne; or of Loch Lomond or Loch Tay.—Time, February 1840.

10. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Natural History and Comparative Anatomy of the Land and Water Molluscous Animals of the Firth of Forth district; Drawings, and, if possible, Preparations to be given in.—Time, February 1841.

11. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Anatomy and Physiology of the Respiratory and Digestive Organs of Birds, founded on actual observation, with a special reference to the habits and manners, and the natural arrangement of Families and Genera. Series of characteristic Specimens required.—Time, February 1843.

BOTANY.

12. TEN SOVEREIGNS, or a piece of plate of that value, for an approved Essay on the Botany of the Mountains of Scotland, in con-

nection with their Geological Structure and Composition; with Specimens, and a Map of the Distribution of the Plants. In this Essay, the range of elevation and the northern and southern limits of the different species should be attended to, and any facts tending to illustrate the geographical distribution of plants carefully recorded. It would also add greatly to the interest of the communication, if it were accompanied with a coloured Geognostical Map of the mountainous districts examined.—Time, February 1844.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Babinet upon the Blue Sun.*—Upon the occasion of Mr Forbes's communication concerning the sun seen red across steam, allow me to inform the Academy, that, in studying the phenomena of meteorological optics, I have not neglected those colours which the sun and moon occasionally assume, of an exceedingly dull tint, and without any surrounding rings. The phenomenon of a red sun may be attributed to a defect in the transparency of the atmosphere arising from vapours, or to any other cause; for the fundamental interval of interferences being much greater for the red than for the blue and violet, these latter are first extinguished, and the obstacles to their transmission are comparatively much greater than to the transmission of the red, as is the case with the reflection from a glass that has been merely smoothed, which always begins with the red. Upon this I may remark, that it is very doubtful if the reddish-brown tinge of smoked rock-crystal is owing to a true colour and not to the exclusion of the lower colours of the spectrum induced by the imperfect transparency of the foreign matter. Another phenomenon, which is much more rare and curious than the red sun, is the *blue sun*, when this luminary is seen of a fine blue tint, though somewhat mixed with white. Our scientific repositories contain some instances of this, and I have myself seen two. It is evident, that the yellow hue, which is much less remarkable on account of its analogy with the white, must also frequently occur, whilst the violet, owing to the difficulty with which it traverses imperfectly diaphanous me-

dia, will but seldom appear. I attribute these colours to the interference of the rays which have traversed the vesicles of water or vapour, with those which have traversed air only. The phenomenon simply implies, that the part of each traversed vesicle is not too thick, a supposition which is easily admitted *a priori.* It is completely of the same nature with that which you have yourself observed in mica or gypsum plates of different thicknesses, and in which the two neighbouring rays which traverse the different thicknesses of the mica or gypsum interfere, and so produce the coloured rays (an experiment which, in a parenthesis, we may observe, has twice been reimported during the last year from England); they are likewise the phenomena known under the term of the *mixed plates* of Dr Young. To produce, then, a blue sun, the red, yellow, and even the violet sun, I have taken (see *Societé Philomatique*, 1827) two plane circular glasses, separated by a layer of mixed water and air, of oil and air, and, finally, of oil and water; and by suitably approximating the glasses, I have made the flame of a lamp to be seen through them of a uniform red tint, and of a blue and violet tint, at pleasure. The enfeebled image of the sun reflected by water assumes the same colours; and the moon still more strikingly, and by direct vision. Hence, then, I imagine nothing requires to be added to the explanation and the reproduction of the meteorological phenomenon.—Not, however, to dismiss the subject of the colours of *mixed plates*, without pointing out some other particulars besides their very uniform tints, I shall remark, that around the flame may be perceived the field of two glasses of a more feeble colour, and complementary to the colour of the flame; a circumstance of which Dr Young, whom I consulted upon it, could not perceive the cause which I have somewhat investigated. I still contend, that these colours differ from the usual colours of their plates, in that these latter, in the oblique incidences, are polarized in the plane of incidence, both as it respects the *transmitted* rays and the *reflected* rays, as you have demonstrated in the *Memoires d'Arcueil;* a circumstance which does not occur in those colours of mixed plates, which are transmitted obliquely, and which are partially polarized *as if by transmission*, that is to say, perpendicularly as to the planes of incidence, of reflection, or of transmission,—so far coinciding. I shall conclude by remarking, that the planes of glass being superimposed one on the other, we can easily give to the mixed plate a suitable thickness, by turning the two glasses upon each other with a moderate pressure, and by heating them.

P. S. I moreover add, that the colours of *mixed plates* do not need, like those of the more ordinary rings, to be placed at the precise distance of distinct vision; that the two interfering rays not possessing the same intensity, there cannot be the complete destruction of any colour, which is equivalent to the declaration that all the tints are more or less mixed with white; and that, finally, as it regards the reflected rings of mixed plates, the centre is *white*, the very opposite of what is observed in ordinary rings, on account of the known loss of a semi-interval of interference.

2. *Influence of the Height of the Barometer on the Level of the Sea.*—At Stockholm, it is a circumstance generally noticed by the people, that, when the water in the harbour, which is a bay of the north sea, stands low, so that the water of the Mälar Lake, which has almost the same level with the mean height of the North Sea, has free exit, the air is clear and dry; and when the reverse is the case, so that the North Sea flows into the Mälar, wind and rain may soon be expected. This circumstance was investigated by N. G. Schultén, who, after he had ascertained the truth of the belief, proposed the following explanation, in the Transactions of the Royal Academy for 1806, p. 77;—that when the height of the barometer, that is, the pressure of the air, is inconsiderable over this part of the North Sea, but higher over another, the equilibrium will be more speedily restored by water than by air, for this reason, that the latter cannot give way to a higher pressure, but must flow away; whereas the elasticity of the air restores the equilibrium in this manner very slowly, owing to the portions of air in contact with each other, of unequal pressure, having so slight a difference in pressure, that it is almost un-

observable, although the total amount of difference at points many miles distant may be very considerable. The level of the water will, therefore, be lower under a higher pressure, and higher under a smaller pressure of the atmosphere, and a tendency to equilibrium will result. These ideas of Schultén, although well known in Stockholm, have not attracted, in other countries, the attention they deserve. Lately, they have received a confirmation from Daussy, Ann. de Chim. et de Phys. 62, p. 304, who, without being acquainted with Schultén's memoir, has made observations at Lorient, on the greatest height of the surface of the sea at high water. These, accompanied by barometrical tables, have been published, and exhibit, in a most complete manner, the phenomenon to which we have now alluded. I consider it unnecessary to quote the details of the observations.—*Berzelius's Jahres-Bericht, Siebenzehnter Jahrgang*, p. 64.

GEOLOGY.

3. *Dr Berendt's Investigations on Amber.*—We learn with much satisfaction, from a letter sent us very recently by Dr Berendt of Dantzig, that his important work on the insects, &c., found imbedded in amber, and which, though commenced in 1830, has been interrupted in its publication, is now to be carried on and completed with as little delay as possible. The first part contains an analytical or rather synthetical account of the *amber tree*, and of the flowers and fruits of other vegetable productions which grew in the amber woods. Dr Berendt has transmitted to us the fifteen lithographed plates illustrative of his second part, and which have greatly interested and surprised us by the richness of the Entomological Fauna they exhibit. These figures represent the Crustacea, Myriopoda, Arachnidæ, and Aptera, examined by the author; and it would appear that all the species found in amber are now extinct, and that but a small number of the genera at present exist. Many new genera have therefore been formed, and also one entirely new family. Of the latter, the species *Archæa paradoxa*, figured in plate 2d, at once arrests the attention by its singular structure and

form. The 3d part is also in preparation, and will include the Hemiptera, Orthoptera, and Lepidoptera; the 4th will contain the Neuroptera, and Hymenoptera; the 5th, the very numerous division of the Diptera: and the 6th and last will contain the equally rich section of the Coleoptera.

4. *Notice regarding the Stone used in constructing the Temples at Pæstum.*—When at Pæstum, on the 3d of June 1838, I observed a fact regarding the travertine of which the splendid Grecian temples there are constructed, which you may perhaps think worth inserting in your Journal, and thus lead future travellers to inquire into the cause of the phenomenon.—We remarked, that the *colour* of the three temples was very different, although they are all built of the same material, (travertine or fresh-water limestone, containing imbedded fresh-water shells); two of them being of a grey colour, and the other, that usually known as the "Temple of Neptune," of a rich yellowish-brown. On closer examination, I found that this difference in the colour was caused by the two former being covered with a grey crustaceous lichen, from which the latter was perfectly free. I was for some time unable to account for this, when it struck me, that it might be caused by the stone containing some matter inimical to vegetation; and on applying a freshly broken piece to the tongue, I perceived a distinctly saline taste.—I brought away specimens of the stone from the different temples, which I gave for examination to my friend Mr Kemp, who informs me, that after reducing portions of it to powder and boiling it in distilled water, he found, in a portion taken from one of the smaller temples, no saline matter in solution; but in that from the largest, or the "Temple of Neptune," a considerable quantity of *muriate* of lime. This fact accounts at once for the absence of vegetation on that building.—It would be interesting to ascertain whence this salt was derived, and I regret much that we had not time to examine the spot where are said to be the ancient quarries, about two miles inland from the ruins, an inspection of which might clear up this point.—*Letter from W. C. Trevelyan, Esq.*

5. *Fossil-Tree at Granton, near Edinburgh.*—A great fossil-tree, similar to that at Craigleith, has been discovered in the sandstone quarry at Granton. Its dimensions cannot yet be ascertained, but the distance between the extreme points already uncovered is about forty-five feet, and its breadth, where most exposed, is about three feet.

6. *Method of distinguishing Trap from Basalt.*—Mons. A. Braconnot finds, that these rocks may be distinguished by subjecting them to distillation; the trap always yields an empyreumatic product, which restores the colour of litmus paper reddened by an acid, whereas basalt produces no such effect; and he presumes that the organic matter which had existed in the materials of the basalt, was destroyed by the volcanic heat by which the rock was produced; whereas, he conceives trap to have been formed in water, under the influence of a moderate temperature, insufficient to destroy the organic matter which was contained in the debris from which it was formed. The trap was selected from various places, and of an unquestionable nature; and the basalt was that of Clermont, in Auvergne. M. Braconnot has also found, that various granites yield ammonia when heated, and the same effect was produced by serpentine and porphyry; but the gneiss of Freiberg, in Saxony, yielded an acid which appeared to be the hydrofluoric. Many other rocks, of various kinds, were subsequently found to yield ammonia.—*Ann. de Chim. et de Phys.*, *January* 1838.

7. *Tornadoes on the West Coast of Africa.*—There are squalls within the limits of the trade winds which evidently are not of the nature of rotatory storms. From explanations received from naval offices, as well as from some log-books, I should be convinced that the tornadoes on the west coast of Africa, as well as the pamperos on the coast of South America, and also arched squalls, are phenomena altogether different from the whirlwind; but this opinion still requires confirmation.—*Reid on Law of Storms.*

NEW PUBLICATIONS.

1. ***Zoology of the Voyage of H. M. Ship Beagle, under the Command of Captain Fitzroy, during the Years* 1832 *to* 1836. 4to. Smith, Elder, and Company, London.**

The only Number of this work published since our last notice at page 433 of former volume (vol. xxvi.), is No. 3. of Part I. of the Fossil Mammalia, by Mr Owen.

Mr Owen, in this Number, concludes his account of the fossil remains of his new subgenus Mylodon with the following summary :—" From the preceding descriptions, it will be seen that the lower jaw of the Mylodon (the only part of the animal hitherto met with), is very different from that of the *Megatherium;* with that of the *Megalonyx* we have at present no means of comparing it. Among existing Edentata, the Mylodon, in the form of the posterior part and angle of the jaw, holds an intermediate place between the Ai and the great Armadillo ; in the form of the anchylosed symphysis of the lower jaw, it resembles most closely the Unau or two-toed sloth ; but in the peculiar external configuration of the symphysis, resulting from the mammilloid processes above described, the Mylodon presents a character which has not hitherto been observed in any other species of *Bruta*, either recent or fossil." The descriptions are illustrated by two plates, executed in the usual style of excellence of this work. To the description of the Mylodon follows that of a considerable part of the skeleton of a large Edentate mammal, allied to the Megatherium and Orycteropus, and for which Mr Owen proposes the name of *Scelidotherium Leptocephalum* (from Σκελις, femur ; θηριον, bellua ; in allusion to the disproportionate size of the thigh-bone). The Cape ant-eater (Orycteropus), of all Edentata, most nearly resembles the present fossil in the form of its cranium, and next in this comparison the great Armadillo (Dasypus gigas, Cuvier), may be cited. Although the Scelidotherium, like most other Edentals, appears to have been of a low stature, and, like the Megatherium, presents a disproportionate development of the hinder parts, it is probable, says Mr Owen, that, bulk for bulk, it equalled, when alive, the largest existing Pachydermis, not proboscidian. There is no evidence that it possessed a tesselated osseous coat of mail. The description, not finished in this Number, is illustrated by nine beautiful plates.

2. *Illustrations of the Zoology of Southern Africa, &c. &c.* By ANDREW SMITH, M.D., Surgeon to the Forces, and Director of the Expedition into the Interior of Africa. 4to. Smith, Elder, and Company, London.

This, which is the fifth and last published number of the Illustrations of the Zoology of Southern Africa, contains excellent coloured representations and accurate descriptions of different new species of quadrupeds and birds, and one undescribed species of fish belonging to the genus Mustelus.

Quadrupeds.—1. Galago Moholi, *Smith.* 2. Chrysochloris villosa, *Smith.* The genus Chrysochloris, as far as is yet known, is confined to Africa. The first discovered species, the C. aurata, excited much attention from the metallic lustre of its fur, but it is not mentioned by Dr Smith that the species so well described and figured by him has the same metallic aspect. It would appear that there are now four species of this genus, known to naturalists, viz. C. aurata, C. Hottentota, *Smith;* C. villosa, *Smith;* and C. Damarensis of *Ogilvy.*

Birds.—1. Astur melanoleucus, *Smith.* 2. Otis Afroides, *Smith.* 3. Gallinula dimidiata, *Smith.* 4. Gallinula Jardinii, *Smith.* 5. Gallinula elegans, *Smith.* 6. Vanellus lateralis, *Smith.*

Fishes.—Mustelus megalopterus.

3. *Illustrations of Mechanics.* By the Rev. H. MOSLEY, F.R.S., Professor of Natural Philosophy and Astronomy in King's College, London. 8vo, pp. 436. Engravings on wood. Longman and Company, London. 1839.

This valuable volume is the first of a series of "Illustrations of Science," by the Professors of King's College, London, in the course of publication by Messrs Longman and Company.

4. *Dictionary of Arts, Manufactures and Mines, containing a clear Exposition of their Principles and Practice.* By A. URE, M.D., F.R.S., &c. Illustrated with 1240 engravings in wood. 8vo, pp. 1344. Longman and Company. 1839.

The printing of this very useful work is now finished. We have only to add to our former commendation, that we doubt not it will ere long find a place in every manufactory and workshop in the country.

5. *Journal of the Asiatic Society of Bengal.* Edited by Mr PRINSEP. Numbers for August, September, October, and November, 1839. Calcutta; and Messrs W. H. Allen and Co., London.

In these numbers we find the following articles immediately connected with physical science:—

August:—Report of the Coal discovered in the Tenasserim Provinces, by Dr Helfer. Note on a species of Arctonix from Arracan, by Dr G. Evans, Curator As. Soc. Museum. *September*:—Botanico-Agricultural account of the protected Sikh States, by M. P. Edgeworth, Esq., C. S. Masuri. Extracts from the Mohit (the Ocean), a Turkish work on Navigation in the Indian Seas, translated and communicated by Professor Joseph Von Hammer. Note on the New Zealand Caterpillars, by G. Evans, Esq. Table of Mortality for ages from birth to twenty years, framed from the Registers of the Lower Orphan School, Calcutta, by H. T. Princep, Esq. *October*:—Report of a Visit made to the supposed Coal-field at Bidjeegurh (Vijayagadha), by Mr George Osborne. Report on ten specimens of Coal from Captain Burnes. Note on the Animal productions of the Tenasserim Provinces, by J. W. Helfer, Esq. M.D. On a new species of Pheasant from Thibet, by B. H. Hodgson, Esq.—Population and Mortality in Calcutta. *November*:—Report on the Copper Mines of Kumaon, by Capt. H. Drummond, 3d B. L. C. Observations on six new specimens of Cyprinidæ, with an outline of a new classification of the family, by J. M'Clelland, Esq. Bengal Medical Establishment. Report upon the Coal-beds of Assam (submitted to Government by the Committee appointed to investigate the Coal and Iron resources of the Bengal Presidency, as a supplement to their first printed Report).

List of Patents granted for Scotland from 18*th March to* 18*th June* 1839.

1. To **Samuel Clegg** of Sidmouth Street, Gray's Inn Road, in the county of Middlesex, engineer, for an invention of "a new improvement in valves, and the combination of them with machinery."—18th March 1839.

2. To **John Leigh** of Manchester, in the county of Lancaster, surgeon, for an invention of "an improved mode of obtaining carbonate of lead, commonly called white-lead."—18th March 1839.

3. To **Joseph Bennet** of Turnlee, near Glossop, in the county of Derby, cotton-spinner, for an invention of "certain improvements in the machinery for carding, drawing, slubbing, roving, and spinning silk, wool, cotton, worsted, flax, and other fibrous substances; which improvements are also applicable to other useful purposes."—20th March 1839.

4. To **John Player**, the younger, of Loughor, near Swansea, in the county of Glamorgan, engineer, for an invention of "improvements in furnaces and fire-places for consuming anthracite and other fuel, for generating steam, evaporation, smelting and heating iron and other metals."—21st March 1839.

5. To **John Robinson** of North Shields, in the county of Northumberland, engineer, for an invention of "a nipping lever for causing the rotation of wheels, shafts, or cylinders, under certain circumstances."—22d March 1839.

6. To **James Gardner** of Banbury, in the county of Oxford, ironmonger for an invention of "improvements in cutting Swedish turnips, mangel-wurzel, and other roots used for food for sheep, horned cattle, and other animals."—25th March 1839.

7. To **George Cotton** of Winsley Street, Oxford Street, in the county of Middlesex, engineer, for an invention of "improvements in the construction of wheels for railway and other carriages."—25th March 1839.

8. To **Richard Roberts** of Manchester, in the county palatine of Lancaster, civil-engineer, an extension of seven years, from 5th April 1839, of a patent granted to him for an invention of "an improvement, or certain improvements of, in, or applicable to the mule, billy, jenny, stretching frame, or any other machine or machines, however designated or named, used in spinning cotton, wool, or other fibrous substances, and in which either the spindles recede from and approach the rollers or other deliverers of the said fibrous substances, or in which such rollers or deliverers recede from and approach the spindles."—25th March 1839.

9. To **Andrew Smith** of Prince's Street, Leicester Square, in the county of Middlesex, engineer, for an invention of "certain improvements in apparatus for heating fluids and generating steam."—27th March 1839.

10. To **Wilton Wood** of Liverpool, in the county of Lancaster, merchant, for an invention of "improved methods of making bands and tackling, to be used in driving, turning, or carrying machinery."—27th March 1839.

11. To **John Ruthven** and **Morris West Ruthven** of Edinburgh, civil-engineers, for an invention of "improvements in boilers for generating steam, in economizing fuel, in propelling vessels by steam or other power, and ventilating vessels, and which may be applied to mines or buildings."—28th March 1839.

12. To **John Gray** of Liverpool, in the county of Lancaster, engineer, for an invention of "certain improvements in steam-engines, and apparatus connected therewith; which improvements are particularly applicable to marine engines for propelling boats or vessels; and part or parts of which improvements are also applicable to locomotive and stationary steam-engines, and other purposes."—29th March 1839.

13. To **William Hale** of Greenwich, in the county of Kent, for an invention of "improvements in steam-engines, and apparatus connected therewith, and in machinery for propelling vessels; part of which improvements are applicable to raising or forcing fluids."—2d April 1839.

14. To **William Henry Porter** of Russia Row, Milk Street, Cheapside, in the city of London, warehouseman, for an invention of "improvements in anchors."—2d April 1839.

15. To **Thomas Adamson** of Dundee, in the county of Forfar, North Britain, ship-builder, for an invention of "certain improvements in the machinery employed in turning windlasses."—2d April 1839.

16. To **Robert Logan** of Trafalgar Square, in the county of Middlesex, for an invention of a "new cloth or cloths constructed from cocoa-nut fibre, and certain improvements in preparing such fibrous material for the same and other purposes."—3d April 1839.

17. To **John Bourne** of the city of Dublin, engineer, for an invention of "certain improvements in steam engines, and in the construction of boilers, furnaces, and stoves."—16th April 1839.

18. To **Job Cutler** of Lady Poole Lane, Spark Brook, in the parish of Aston, in the borough of Birmingham, in the county of Warwick, gentleman, for an invention of "improvements in combination of metals applicable to the making of tubes or pipes, and to other purposes, and in the method or methods of making tubes or pipes therefrom; which improved method or methods is or are applicable to the making of tubes or pipes from certain other metals and combinations of metals."—17th April 1839.

19. To **John Woolrich** of Birmingham, in the county of Warwick, professor of Chemistry, for an invention of "an improved process of manufacturing carbonate of lead, commonly called white lead."—17th April 1839.

20. To **John Hillard**, for an invention, communicated by a foreigner residing abroad, of "certain improvements in machinery or apparatus for making or manufacturing screws."—24th April 1839.

21. To **James Nasmyth** of Patricroft, near Manchester, in the county of Lancaster, engineer, for an invention of "certain improvements applicable to the bearings or journals of locomotive and other steam-engines; which improvements are also applicable to the bearings or journals of machinery in general."—24th April 1839.

22. To **George Price** of Cornhill, in the city of London, gentleman, for an invention communicated by a foreigner residing abroad, of "im-

provements in filtering and clarifying water and other liquids."—30th April 1839.

23. To **William Pontifex** of Shoe Lane, in the city of London, coppersmith, for an invention of "improvements in apparatus or materials employed in filtering and clarifying water and other liquids."—30th April 1839.

24. To **James Smith** of Deanston Works, in the parish of Kilmadock, in the county of Perth, cotton-spinner, for an invention of "certain improvements in the machinery for spinning and twisting of wool and other similar fibrous substances."—30th April 1839.

25. To **Thomas Barnabas Daft** of Regent Street, in the county of Middlesex, gentleman, for an invention of "certain improvements in inkstands, and in materials and apparatus for fastening and sealing letters."—30th April 1839.

26. To **Elisha Haydon Collier**, late of Boston, in the United States of North America, but now of Globe Dock Factory, Rotherhithe, in the county of Surrey, civil-engineer, for an invention of "improved machinery for manufacturing nails."—30th April 1839.

27. To **Mathew Heath** of Furnival's Inn, gentleman, for an invention, communicated by a foreigner residing abroad, of "improvements in clarifying and filtering water, beer, wine, and other liquids."—30th April 1839.

28. To **James Whitelaw** of Glasgow, in the county of Lanark, North Britain, engineer, for an invention of "an improved rotatory machine to be worked by the pressure and reaction of a column of water, which machine may be used as a steam-engine, also an improved water meter, and a machine for raising water or other liquid by its centrifugal force."—17th May 1839.

29. To **William Watson** of Temple Street, in the county of Dublin, gentleman, for an invention of "an improvement in the construction of ships, and which improvement is also applicable to all kinds of sea-going vessels; and also certain improvements in the construction of boats and other vessels intended to be used on canals and inland navigations."—20th May 1839.

30. To **Joseph Amesbury** of Burton Crescent, in the parish of St Pancras, in the county of Middlesex, surgeon, for an invention of "a certain apparatus, or an improvement or improvements in apparatus for the support of the human body."—24th May 1839.

31. To **John Henfrey** of Weymouth Terrace, in the parish of Shore Ditch, and county of Middlesex, engineer and machinist, for an invention of "certain improvements in the manufacture of hinges or joints, and in the machinery employed therein."—29th May 1839.

32. To **John Boyd** of College Street, South, and **Hugh Francis Rennie** of Glengall Street, both in the town of Belfast, and in the county of Antrim, flax-spinners, for an invention of "certain improvements upon the spinning-frame used for spinning flax, hemp, and tow, upon the wet principle."—29th May 1839.

33. To **John Williamson Whittaker** of Bolton, in the county of Lan-

caster, joiner, and **Rowland Hall Heaton**, of the same place, cotton-spinner, for an invention of "certain improvements in the means of connecting or uniting straps or bands for driving machinery and other similar purposes, and in the apparatus for effecting the same."—30th May 1839.

34. To **James Drew** of Manchester, in the county of Lancaster, civil-engineer, for an invention of "certain improvements in the means of consuming smoke and economizing fuel in steam-engines or other furnaces or fire-places."—31st May 1839.

35. To **John George Bodmer** of Manchester, in the county of Lancaster, engineer, for an invention of "certain improvements in the machinery or apparatus for carding, drawing, roving, and spinning cotton, wool, flax, silk, and other fibrous substances."—5th June 1839.

36. To **Edward Pearson Tee** of Barnsley, in the county of York, dyer and linen-manufacturer, for an invention of "improvements in weaving linen and other fabrics."—6th June 1839.

37. To **William Hickling Burnett** of Wharton Street, Bagnigge Wells Road, in the county of Middlesex, gentleman, for an invention of "improved machinery for cutting or working wood."—12th June 1839.

38. To **John Roberts** of Manchester, in the county of Lancaster, machine-maker, for an invention of "certain improvements in machinery or apparatus for planing or cutting metals."—14th June 1839.

39. To **George Nelson** of Leamington Priors, in the county of Warwick, gentleman, for an invention of "a certain new or improved method, or new or improved methods, of preparing gelatine, which has the properties of or resembles glue."—17th June 1839.

40. To **William Morgan** of New Cross, in the county of Surrey, gentleman, for an invention of "improvements in the generation of steam."—17th June 1839.

41. To **William Whitham** of Huddersfield, in the county of York, machinist, for an invention of "improvements in engines to be worked by steam, water, or other fluids."—17th June 1839.

42. To **Alexander Francis Campbell** of Great Plumstead, in the county of Norfolk, Esq., and **Charles White** of the city of Norwich, mechanic, for an invention of "certain improvements in ploughs, harrows, scarifiers, cultivators, and horse-hoes.—17th June 1839.

43. To **Charles Andrew Caldwell**, of Audley Square, in the county of Middlesex, Esq., for an invention, communicated by a foreigner residing abroad, of "improvements in furnaces and apparatus for applying the heat of fuel."—18th June 1839.

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8. An Etymological and Explanatory Dictionary of the Terms and Language of Geology. By GEORGE ROBERTS, 12mo, pp. 183, ... 435

9. Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology, and as a Guide to the philosophical pursuit of Natural History. By W. B. CARPENTER, M.D., Member of the College of Surgeons, London, &c. 8vo, pp. 478. Six Plates, ... 435

XX. List of Patents granted for Scotland from 25th June to 17th September 1839, . . . 437

NOTICES.

I. We are requested by Dr Martin Barry to state, that, at the time of presenting his "Second Series" of "Researches on Embryology" to the Royal Society (of which an abstract was given in our last Number), he was not aware that the fact regarding Spermatozoa reaching the surface of the Ovary, had been previously discovered by Dr Bischof of Heidelberg.

II. The important Biographical Memoir of Watt, occupying so large a portion of the present number of the Journal, obliges us to delay till our next publication, several communications we intended to have laid before our readers at this time.

THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

Biographical Memoir of JAMES WATT, *one of the Eight Foreign Associates of the Academy of Sciences.* By M. ARAGO, Perpetual Secretary.*

GENTLEMEN,—After perusing the long list of battles, assassinations, plagues, famines, and direful calamities of all sorts, which the annals of some country presented, a philosopher exclaimed, "Happy the country whose history is uninteresting!" To this apophthegm another may, with great propriety, be added, at least in a literary point of view, namely, "Hard the task of the man who is called to recount the history of a happy people!" If the exclamation of the philosopher lose nothing of its force when applied to individuals, its counterpart frequently, with equal truth, characterizes the labour of the biographer. Such has been the nature of my reflections while studying the life of James Watt, and while collecting the kind communications of the relatives, the acquaintances, and the friends of the illustrious mechanist. That life, devoted to labour, to study, and to meditation, furnishes none of those striking events whose recital, thrown with a little address among the details of science, tempers their dulness. I shall, however, relate it, were it for no other reason than to shew in how humble a sphere were elaborated those mighty projects which were destined to elevate the British nation to an unheard of height of power; and I shall endeavour more particularly to characterize with minute accuracy those splendid inventions which will for ever associate the name of Watt with the steam-engine. I am well aware of the disadvantages which attend this plan, and am prepared for the criticism: "We expected an historical eloge, and have heard only a dry and barren lecture." If my discourse, however, be comprehended, I shall willingly submit to the reproach. I shall do my best not to fatigue your attention, remembering that clearness constitutes politeness in a public speaker.

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* Several years ago (Dec. 8. 1834), when this Memoir was read to the French Academy of Sciences, we applied for a copy, with the view of translating it for our Journal, where already so many of the most classical of the Eloges of Cuvier, Arago, and Flourens had appeared. Our wish was promised to be gratified, but, owing to considerable delay in printing, our copy, although the first sent to this country, was not received till lately. Being aware of the universal interest which this remarkable Memoir was to excite, we have not delayed our translation, and have now the pleasure of laying before our readers a faithful, and, we trust, accurate version of the most important Eloge ever written by Arago.

Childhood and youth of JAMES WATT*; his appointment as Instrument-maker to the University of Glasgow.*

JAMES WATT, one of the eight Foreign Associates of the Academy of Sciences, was born at Greenock in Scotland on the 19th day of January in the year 1736. Our neighbours on the other side of the Channel have the good taste to think that the genealogy of a respectable and industrious family is as worthy of preservation as the musty parchments of some titled houses which have become celebrated only by the enormity of their vices or their crimes. Hence it is that I can state with certainty that the great-grandfather of James Watt was a farmer settled in the county of Aberdeen; that he fell in one of Montrose's battles; that the conquering party, as was then the custom (and I was going to add as is still the practice in civil broils), did not consider death as sufficient expiation for those opinions on account of which the poor farmer had bled; that they, moreover, punished him in the person of his son, by confiscating his small property; that this unfortunate child, whose name was Thomas Watt, was taken care of by some distant relations; that, in the very isolated situation to which he was thus reduced, he devoted himself to serious and assiduous study; that, in more tranquil times, he established himself in Greenock, where he taught mathematics and the elements of navigation; that he resided in the Burgh of Barony of Crawford's-dyke, of which, for several years, he held the office of baron-bailie, or, in other words, was chief magistrate; and that, finally, he died in the year 1734, at the age of ninety-two years.*

Thomas Watt had two sons. The elder, John, followed, in the city of Glasgow, the occupation of his father. He died in the year 1737, at the age of fifty, having executed a chart of the course of the river Clyde, which was subsequently published under the direction of his brother *James.* This last named individual, the father of the celebrated engineer, was, during the quarter of a century, councillor, treasurer, and bailie of Greenock, having declined the office of chief magistrate, and was celebrated for the ardent zeal, and the enlightened spirit of improvement, with which he discharged his duties. He was a pluralist (and let no one be alarmed at these three syllables, which have now in France become the object of general anathema; they injure not the memory of James Watt), —he combined three different kinds of occupation; he furnished the several kinds of apparatus, utensils, and instruments which are necessary for navigation; he was also a builder and a merchant; notwithstanding which, towards the close of life, he unfortunately suffered severely from some commercial enterprizes which deprived him of a portion of that honourable fortune he had previously acquired. He died at the age of eighty-four, in the year 1782.

James Watt, the subject of this Memoir, was in infancy an exceedingly delicate child. His mother, whose family name was Muirhead, was his first instructor in reading, whilst his father taught him writing and arithmetic. He also attended the elementary public school at Greenock; so that the humble *grammar schools* of Scotland may boast of having educated the celebrated engineer, in the same way that the *Collège de la Flèche* was wont to enumerate Des Cartes; and Cambridge to the present day prides itself on Newton. To be minutely accurate, however, I ought to add, that habitual indisposition interfered with young Watt's regular attendance at the public school of Greenock; and that, for a great part of the year, he was confined to his chamber, where he devoted himself to study, without any extrinsic aid. As often happens, his superior intellectual faculties, destined to produce such valuable results, began to develope themselves in retirement and meditation.

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* On his gravestone he is designated "Professor of Mathematics."—Edit.

Watt was so delicate that his parents did not venture to impose any thing in the shape of severe tasks upon him; they left him very much at liberty in the choice of his occupations, and it will be seen he did not abuse the indulgence. A gentleman one day calling upon Mr Watt, observed the child bending over a marble hearth, with a piece of coloured chalk in his hand; "Mr Watt," said he, "you ought to send that boy to a public school, and not allow him to trifle away his time at home." "Look how my child is employed, before you condemn him," replied the father. The gentleman then observed that the child had drawn mathematical lines and circles on the hearth. He put various questions to the boy, and was astonished and gratified with the mixture of intelligence, quickness, and simplicity displayed in his answers: He was then trying to solve a problem of geometry. Influenced by his parental solicitude, Mr James Watt very early put a number of tools at the disposal of the young scholar, who very soon used them with the greatest possible address. He would take to pieces and again put together the various toys that came within his reach, and he was very active in making new ones. Somewhat later he undertook the construction of a small electrical machine, whose brilliant sparks became a lively source of amusement and surprise to his young companions.

Watt, with an excellent memory, might, nevertheless, not have peculiarly distinguished himself among the youthful prodigies of ordinary schools. He never could have learned his lessons like a parrot, for he experienced a necessity of carefully elaborating the intellectual elements presented to his attention, and nature had peculiarly endowed him with the faculty of meditation. Upon the whole, Mr James Watt augured most favourably of the nascent powers of his child. Some other of his more distant relatives, less discerning, did not share in these hopes. His aunt Mrs Muirhead, sitting with him one evening at the tea-table, said, "James, I never saw such an idle boy! Take a book or employ yourself usefully. For the last half hour you have not spoken a word, but taken off the lid of that kettle and put it on again, holding now a cup and now a silver spoon over the steam; watching how it rises from the spout, and catching and counting the drops of water formed by condensation." It appears that when thus blamed his active mind was engaged in investigating the condensation of steam.

Who among us, if we had been placed in the same circumstances as Mrs Muirhead, would not in the year 1750 have resorted to the same language? But the world since that time has advanced, and our knowledge has increased. Moreover, when I shall speedily explain that the principal discovery of our associate consisted in a particular method of converting steam into water, Mrs Muirhead's reproaches will appear to us in a very different light; the boy pondering before the tea-kettle will be viewed as the great engineer preparing discoveries which were soon to immortalize him; and it cannot but appear remarkable that the words *condensation of steam* should come, as it were, naturally to present themselves in the history of the infancy of Watt. I have the more willingly alluded to this singular anecdote, because, for its own sake, it richly merits preservation. And, as the occasion has presented itself, let us impress on youth that it was not modesty alone which prompted the response of Newton, when, in reply to a certain great personage who inquired how the principle of gravity was discovered, he answered, "*By always thinking of it!*" Let us point out, in these simple words of the great author of the *Principia*, what is the true secret of men of genius.

The extraordinary felicity of anecdote with which our associate, for fifty years, delighted all those with whom he associated, very early developed itself. The proof of this will be found in a few lines which I extract from an unpublished note written in the year 1798 by Mrs Marion Campbell, the cousin and youthful companion of the celebrated engineer.* "He was not fourteen, when his mother brought him to Glasgow to visit a friend of hers; his brother John accompanied him. On Mrs Watt's return to Glasgow some weeks after, her friend said, "You must take your son James home; I cannot stand the degree of excitement he keeps me in, I am worn out for want of sleep. Every evening before ten o'clock, our usual hour of retiring to rest, he contrives to engage me in conversation, then begins some striking tale, and, whether humorous or pathetic, the interest is so overpowering, that the family all listen to him with breathless attention, and hour after hour strikes unheeded."

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* I am indebted for this curious document to my friend Mr James Watt of Soho. Thanks to the profound veneration he has preserved for the me-

James Watt had a younger brother, John,* who, having determined to follow the career of his father, left the other, according to the Scottish custom, at liberty to indulge his own taste in selecting his profession. In the present case, however, this was unusually difficult, for the young student prosecuted almost every branch of science with equal success. The banks of Loch Lomond, already so celebrated by the recollections of the historian Buchanan, and by those of the illustrious inventor of Logarithms, developed his taste for the beauties of nature and for botany. His rambles among the mountain-scenery of Scotland made him perceive that the inert crust of the globe was not less worthy of attention, and he became a geologist. James also took advantage of his frequent intercourse with the humbler classes in those enchanting regions, for the purpose of decyphering their local traditions, their popular ballads, and their wild prejudices. When his state of health confined him to his father's dwelling, it was chiefly chemistry which formed the subject of his investigations. s'Gravesande's *Elements of Natural Philosophy* initiated him also into the thousand marvels of general physics; and finally, like all valetudinarians, he devoured such

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mory of his illustrious father, and still more to the exhaustless kindness with which he has answered all my inquiries, I have through his means been able to avoid various inaccuracies which have found their way into the most esteemed biographies, and which I myself, from partial information, had not been able at first to avoid.

* He perished in one of his father's vessels when sailing from Greenock to America, in 1762, aged 23.

works on medicine and surgery as he could procure. These last sciences had so much excited his interest, that he was one day detected conveying into his room the head of a child which had died of some obscure disease, that he might take occasion to dissect it.

Watt, however, did not devote himself either to botany or to mineralogy, to literature or poetry, or chemistry, or physics, or medicine, although he was so well prepared for the prosecution of any one of these various studies. In the year 1755 he went to London, and there placed himself under the instructions of *Mr John Morgan*, mathematical and nautical instrument-maker in Finch Lane, Cornhill. The man who was about to cover England with engines, in comparison with which, so far at least as effects are concerned, the antique and colossal machine of Marly is but a pigmy, commenced his career by constructing, with his own hands, instruments which were fine, delicate, and fragile,—those small but admirable reflecting sextants to which navigation is so much indebted for its progress. He did not continue with Mr Morgan much above a twelvemonth, and " in the year 1757 went to settle in Glasgow, as a maker of mathematical instruments; but being molested by some of the corporations, who considered him as an intruder on their privileges, the University protected him, by giving him a shop within their precincts, and by conferring on him the title of mathematical instrument maker to the University."* There are still in existence some small instruments which were at this time made entirely by Watt's own hands, and they are of very exquisite workmanship. I may add, that his son has lately shewn me the first designs of the steam-engine, and they are truly remarkable for the delicacy and precision of the drawing. It was not without reason, whatever may be said of it, that Watt spoke, with complacency of his manual dexterity. Perhaps you will think me over scrupulous in thus claiming for our associate a merit which adds but little to his glory. But I confess that I never listen to a pedantic enumeration of the qualities of which able men have been destitute, without thinking of that would-be general in Louis XIV.'s time, who always carried his right shoulder high, because Prince Eugene had this deformity, and imagined that imitating him in this point, it was unnecessary to carry the resemblance any farther.

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* MS. of Dr Black.

Watt had scarcely attained his twenty-first year when he was thus connected with the University of Glasgow. His principal friends on the occasion were Adam Smith, the author of The Wealth of Nations; Dr Black, whose discoveries respecting latent heat and the carbonate of lime have placed him among the first chemists of the eighteenth century; and Robert Simson, the celebrated restorer of the most important works of the ancient geometricians.* These eminent men at first only considered that they had relieved from the vexatious annoyances of the corporations, an expert, zealous, and agreeable workman; but they soon discovered that he was, moreover, a remarkable man, and expressed towards him the warmest friendship. The youth attending the University also considered it an honour to be admitted to his intimacy; so that *his shop*—I repeat, his shop became a kind of academy whither the most eminent persons in Glasgow resorted, to talk over the most difficult questions of art, science, and literature. Nor, in truth, should I venture to describe to you the part that the young workman of twenty-one took in these discussions, if I could not do so in the unpublished words of one of the most illustrious contributors to the Encyclopædia Britannica. "I had always, from my earliest youth," writes the late Professor Robison, "a great relish for the natural sciences, and particularly for mathematical and mechanical philosophy, when I was introduced by Drs Simson, Dick, and Moor, gentlemen eminent for their mathematical abilities, to Mr Watt. I saw a workman, and expected no more; but was surprised to find a philosopher, as young as myself, and always ready to instruct me. I had the vanity to think myself a pretty good proficient in my favourite study, and was rather mortified at finding Mr Watt so much my superior. . . . . Whenever any puzzle came in the way of any of the young students, we went to Mr Watt. He needed only to be prompted; for every thing became to him the beginning of a new and serious study, and we knew that he would not quit it till he had either discovered its insignificancy, or had made something of it. He learnt the German language in order to peruse Leopöld's Theatrum machinarum. So did I, to know what he was about. Similar reasons made us both learn the Italian language. * * * When to his superiority of knowledge is added the *naïve* simplicity and candour of Mr Watt's character, it is no wonder that the attachment of his acquaintances was strong. I have seen something of the world, and am obliged to say I never saw such another instance of general and cordial attachment to a person whom all acknowledged to be their superior. But that superiority was concealed under the most amiable candour, and a liberal allowance of merit to every man. Mr Watt was the first to ascribe to the ingenuity of a friend, things which were nothing but his own surmises followed out and embodied by another. I am the more entitled to say this, as I have often experienced it in my own case."[*] It is for you, gentlemen, to determine whether it was not as honourable to have expressed this concluding sentiment as to have inspired it.

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* To these it is only an act of justice to add Dr Dick, Professor of Natural Philosophy, of whose merits Professor Robison and Watt always spoke in terms of eulogy.—EDITOR.

The diversified and profound studies in which the circumstances of his singular position unceasingly engaged the young artisan of Glasgow, were never allowed to interfere with the labours of the workshop. These he executed during the course of the day, whilst the night was devoted to theoretical researches. Confiding in the resources of his fertile imagination, Watt appeared to luxuriate in the most difficult undertakings, and in those which might be thought most foreign to his tastes. Will it be believed that he undertook the building of an organ, though totally insensible to the charms of music, so much so that he could not distinguish one note from another? Nevertheless the work was accomplished. It is scarcely necessary to say, that the new instrument exhibited important improvements in the mechanical details,—in the regulators, and in the manner of measuring the force of the wind; but one is surprised to learn, that its powers of harmony were not less remarkable, and that they delighted professional musicians.

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[*] MS. of the late Professor Robison.

Watt, in fact, resolved an important part of a very difficult problem; he made out the scale of *temperament* by the method of pulsations (des battements), at that time little understood, and the knowledge of which he could not have obtained except in the profound but very obscure work of Dr Robert Smith of Cambridge.

History of the Steam-Engine.

We are now arrived at the most brilliant period of the life of Watt; and I fear also at the most difficult part of my task. The immense importance of the inventions of which I am about to treat, cannot for a moment be doubted; but possibly I may not succeed in making them clearly understood, without going into minute numerical comparisons. That these comparisons, if they do become indispensable, may be readily followed, I shall here state, as briefly as possible, the abstract physical principles upon which they must be based.

As the result of simple change of temperature, water may exist in three perfectly distinct states,—in the solid, the liquid, and the gaseous state. Below 32° Fahr. water becomes ice, at 212° it is rapidly transformed into vapour, and in all the intermediate degrees it is liquid.

The careful observation of the points of passage from one of these conditions into another, leads to discoveries of the highest importance, which form the key to the economical appreciations of steam-engines.

Water is not necessarily warmer than is every kind of ice; water may be maintained at the temperature of 32° without freezing; ice may continue at 32° without melting; but it is very difficult to believe that this water and this ice, both of them at one and the same degree of temperature, differ only in their physical properties, and that there is not some element, apart from water properly so called, which distinguishes the solid water from the liquid water. A very simple experiment will elucidate this mystery. Mix two pounds[*]

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[*] In this illustration, the fractions arising from the differences of the thermometric scales are omitted, and hence the figures are only approximatively correct.—EDITOR.

of water at the freezing point with two pounds at 167° Fahr., the four pounds of the mixture will be found to be at 99°, that is to say, at the mean temperature of the commixed liquids. The hot water is thus found to have preserved 67° of its previous temperature, and to have yielded 67 other degrees to the cold water. All this is what would readily be expected, and could easily be foreseen. And, now, let us repeat the experiment with a single modification. Instead of the two pounds of water at the freezing point, let us take two pounds of ice at precisely the same temperature. From the mixture of this two pounds of ice with the two pounds of water at the temperature of 167°, there will result four pounds of liquid water, since the ice, plunged into the hot water, must needs be dissolved, and will yet retain its former weight; but you must not conclude that, from this second mixture, there will result as from the former a temperature of 99°. Very far from it; in this latter experiment, the water will not be above the freezing point, and there will not remain a single trace of the 135° of the heat of the two pounds of water; these 135° will have dissolved all the particles of the ice, and have combined with them, but without having heated them in the slightest degree.

I have no hesitation in adducing this experiment of Dr Black's as one of the most remarkable in modern physics. Observe its consequences. Ice, at its habitual temperature 32°, and water at the same temperature, differ in their essential composition. The liquid, in addition to what is contained in the solid, includes 135° of an imponderable body which is called *caloric.* These 135° are so thoroughly concealed in the compound, I was about to say the watery alloy, that the most delicate thermometer cannot detect its existence. Hence then, caloric, which is not discoverable by our senses, and which cannot be detected by the most delicate instruments,— in short, *latent heat,* for that is the name which has been bestowed upon it, forms one of the constituent principles of bodies.

The comparison of boiling water, that is to say of water at 212°, with the steam which issues from it, and whose temperature is also 212°, leads to analogous results, but upon a much grander scale. At the moment that steam, at the temperature of 212°, is produced, the water, at the same temperature of 212°, impregnates itself,—*under the form of latent caloric*,—under a form quite insensible to the thermometer,—with an enormous quantity of heat. Again, when the steam reassumes the liquid state, this caloric of composition is disengaged, and goes to heat every thing in its way which is susceptible of absorbing it. If, for example, we were to cause two pounds of steam at 212° to pass through ten pounds of water at the freezing point, the steam would be wholly liquefied, and the twelve pounds which would result from the mixture would be found at the temperature of 212°. Into the intimate composition of two pounds of steam, there enters therefore a quantity of latent caloric sufficient to raise two pounds of water, whose evaporation is prevented, from the freezing point to the height of 995° Fahr. This result will without doubt appear enormous, but it is quite certain. Steam exists only upon this condition. Wherever two pounds of water at the freezing point are evaporated, whether naturally or artificially, in undergoing the transformation, they must seize upon, and in fact do seize upon, 995° of caloric derived from surrounding objects. This number of degrees (for it cannot be too often repeated), the steam entirely restores to the surfaces of whatever nature, upon which the condensation is ulteriorly effected. And here, we may remark in passing, is the whole secret connected with the art of heating by steam. That individual would have a very erroneous idea of this ingenious contrivance, who supposed that the steam conveyed to a distance in the tubes in which it circulates, nothing more than sensible or thermometric heat. The chief effects are, beyond all doubt, owing to the caloric of composition—the hidden or latent heat—which is disengaged at the moment when the contact of the cold surfaces converts the vapour from the gaseous into the liquid state. We must therefore rank caloric among the constituent principles of steam. Caloric is obtained only by the combustion of wood, coal, &c. Steam therefore has a commercial value superior to that of the liquid, by the whole price of the combustible employed in the process of vaporization. If the difference of these two values be very great, it is to be attributed mainly to the latent caloric; the thermometric and sensible heat forms but a small portion of it.

I shall probably have occasion, in the sequel, to return to some of the other properties of steam. If I do not insist upon them at present, it assuredly is not because I attribute to this assembly the state of mind of certain students, who one day observed to their mathematical professor, "Why are you taking all this trouble to demonstrate these theorems? We repose entire confidence in you; give us only your *word of honour*, and that will suffice." But I feel anxious not to abuse your patience; and I ought also to remember that, by referring to particular treatises, you will readily supply the omissions which I shall find it impossible to avoid.

Let us now endeavour to assign the share of merit which is due to the several nations and individuals who should be enumerated in the history of the steam-engine, and trace the chronological series of improvements which this machine has undergone, from its first rude conceptions, now somewhat antiquated, down to the discoveries of Watt. I approach this inquiry with the firm determination of being impartial,—with the most earnest solicitude to bestow on every improver the credit which is his due,—and with the fullest conviction that I am a stranger to every consideration unworthy of the commission you have conferred upon me, or beneath the dignity of science, originating in national prejudices. I declare, on the other hand, that I esteem very lightly the innumerable decisions which have already emanated from such prejudiced sources; and that I care, if possible, still less for the bitter criticisms which undoubtedly await me, for the past is but the mirror of the future.

A question well propounded is half answered. If this sentiment, so full of truth, had always been kept in mind, the discussions concerning the invention of the steam-engine would assuredly never have presented that character of acrimony and violence which has hitherto been stamped upon them. By seeking to discover a single inventor, where it was necessary to recognise many, we have been "in endless mazes lost." The watchmaker who is most deeply versed in the history of his art, would remain dumb before the man who would ask him in general terms, who was the inventor of the watch. The question, on the contrary, would occasion him but little embarrassment, if directed separately to the spring, to the different forms of the escapement, to the balance wheel, &c. So it is with the steam-engine: it now exhibits the realization of various capital but wholly distinct ideas, which could not have emanated from the same source, and of which it is now our duty to search carefully for the origin and the date.

If to have employed steam in any way whatever confers a right, as has been pretended, to figure in this history, we must cite the Arabians in the first rank, since, from time immemorial, their principal food, the pudding, which they call *couscoussou*, is boiled by the action of steam in drainers placed upon their rude pots. Such a conclusion shews only the absurdity of the principle from which it is deduced.

Our compatriot Gerbert, who afterwards wore the triple crown under the name of Silvester II.,—had not he, it may be inquired, a superior claim, when, in the ninth century, he made the organ of the Rheims Cathedral resound by means of steam? I think not. For in the instrument of the future Pope, I see nothing more than a current of steam substituted for the common current of air, so producing the musical phenomena of the organ pipes, but without accomplishing any mechanical effect properly so called.

I find the first example of motion produced by steam in a toy much more ancient than the organ of Gerbert, viz. in the *Eolipyle* of Hero of Alexandria, the date of which ascends to 120 years before our era. It will perhaps be difficult, without the help of figures, to give a clear conception of the mode in which this little piece of apparatus acts; but I shall nevertheless make the attempt. When a gas escapes in a given direction from the vessel which contains it, this vessel has a tendency to move in a diametrically opposite direction, owing to the force of the reaction. The recoil of a musket when discharged is nothing more than this. The gas produced by the inflammation of the saltpetre, charcoal, and sulphur, issues into the air in the direction of the barrel; that direction prolonged backwards, abuts at the shoulder of the person who has discharged it, and it is upon the shoulder therefore that the gun-stock acts with violence. To change the direction of the recoil, all that is necessary is to change the direction in which the gas issues. If the barrel, closed at its extremity, were pierced by a lateral opening at right angles to its direction and horizontal, it would be laterally and horizontally that the gas of the powder would escape,—it would be at right angles that the barrel would produce its recoil,—and it would be upon the arm, and not upon the shoulder, that it would be felt. In the former instance, the recoil operates upon the individual who has fired the piece, from before backwards, with a tendency to throw him down; in this latter instance, it would have a tendency to make him whirl round upon himself. Were we then to attach the barrel, constantly and in a horizontal position, to a moveable vertical axis, at the moment of being discharged it would more or less change its direction, and would cause that axis to revolve upon itself. Still maintaining the same arrangement; suppose now, that the vertical rotative axis is hollow, but closed at its upper extremity, that it abuts at its lower extremity, like a sort of chimney in a boiler producing steam, and that, moreover, there exists a free lateral communication between the interior of the axis and the interior of the gun-barrel, so that the steam, after having filled the axis, penetrates into the barrel, and issues from it at the side by its horizontal opening,—then this steam, in escaping, will act, except in the degree of its intensity, in the same manner as the gas disengaged from the powder in the gun-barrel which was stopped at its extremity and pierced laterally; only that, in this latter instance, we shall not have a simple shock merely, as happens in the violent and instantaneous explosion of the musket, but, on the contrary, the rotatory motion will be uniform and continuous, as the cause which produces it: and finally, were we, instead of having a single musket, or rather a single horizontal pipe, to adapt a number of them to a rotatory vertical tube, then we should have before us, with some unessential differences, the ingenious apparatus of Hero of Alexandria.

Here then, beyond doubt, is a machine in which steam produces motion, and may cause mechanical effects of some importance; in fact, it is a true steam-engine. It is scarcely, however, necessary to add, that it has no real resemblance, either as regards form, or the mode of action of the moving force, to the machines which are now in use. Were, however, the reaction of a current of steam ever to become practically useful, it would unquestionably be right to trace the idea back as far as Hero; at the present day, the rotatory Eolipyle can be introduced here, only as wood-engraving is mentioned in the history of printing.*

In the steam-engines of our manufactories, our steam-vessels, and railroads, the motion is the immediate result of the elasticity of the steam. Hence we must inquire where and how the idea of this power took its origin.

The Greeks and Romans were not ignorant that steam might acquire a prodigious mechanical power; for they explained, with the help of the sudden vaporization of a certain quantity of water, those frightful earthquakes which in a few moments force the ocean from its wonted bed, overturn to their foundations the most solid monuments of human industry, suddenly produce considerable islands in the midst of deep seas, and uplift high mountains in the centre of continents.

Though the contrary has been asserted, it does not appear that this theory of earthquakes presupposes that those who were its authors had engaged in any calculations, experiments, or exact measurements. Every one at the present day, knows the fact, that if at the moment a boiling metal enters into the earthy or plaster moulds of the founder, but a few drops of liquid be enclosed in the moulds, the most dangerous explosions follow. Notwithstanding the progress of science, founders even now a-days do not always escape these accidents; and we have no reason to suppose that the ancients were free from them. While casting, then, their innumerable statues, the splendid ornaments of their temples and public resorts, of their gardens and private mansions at Athens and at Rome, many accidents must have occurred; the artists themselves must have discovered the immediate cause; whilst the philosophers on their part, following that tendency to generalization which was the characteristic feature of their schools, would here behold in miniature a true image of the eruptions of Etna. Now, all this might be true without having the slightest relation to the history with which we are now engaged; and I would not even so far have insisted upon these slight traces of the science of the ancients regarding the power of steam, were it not that I might live at peace with the Daciers of both sexes, and with the Dutens of our own day.*

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* These remarks apply likewise to the project which Branca, an Italian architect, published at Rome in the year 1629, in a work entitled *The Machine*, and which consisted in producing a movement of rotation by directing the steam issuing from an eolipyle, under the form of a blast or wind, upon the winglets of a wheel. If, contrary to all probability, steam should ever be employed in the form of a direct blast, Branca, or the now unknown author from whom he borrowed this idea, would take the first place in the history of this new kind of machines. As it respects those at present in use, the claims of Branca are absolutely null.

Powers, whether natural or artificial, previous to becoming really useful to mankind, have almost always wrought wonders in support of superstition; and steam has been no exception to this general rule.

Chroniclers have informed us, that upon the banks of the Weser, the god of the ancient Teutonic race manifested his displeasure by a kind of thunderbolt, to which, immediately afterwards, succeeded a cloud that filled the sacred enclosure. The image of the god *Busterich*, discovered, it is said, in some excavations, clearly demonstrated the mode in which this prodigy was produced. The god was made of metal. The hollow head contained water to the amount of an amphora, plugs of wood closed the mouth and another opening situated under the forehead, and combustibles suitably placed in a cavity of the cranium gradually heated the liquid. Speedily the steam generated caused the plugs to spring with a loud report, and then escaped with violence, forming a thick cloud between the god and his astonished worshippers. It appears also, that, in the middle ages, the monks found this to be a very valuable invention, and that the head of Busterich has performed before other assemblies besides those of the benighted Teutones.*

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* Influenced by the same motive, I can scarcely avoid mentioning here an anecdote, which, besides its romantic character, and its inconsistency with what we now know of the mode of the action of steam, shews us also the high idea which the ancients entertained of the power of this mechanical agent. It is stated that Anthemius, the architect of Justinian, had a dwelling contiguous to that of Zeno, and that, to annoy the orator, who was his declared foe, he placed beneath the ground-floor of his own house a number of great caldrons, which he filled with water; that from an opening made in the lid of each of these, proceeded a flexible tube, which was directed into the partition wall, under the beams that supported the ceilings of Zeno's mansion; and finally, that these ceilings actually shook as if from a violent earthquake when fires were lit beneath the caldrons.

After these faint glimmerings of the Greek philosophers, we must pass over an interval of nearly twenty centuries, before we meet with any useful notions concerning the properties of steam. From that time onwards, experiments, precise, conclusive, and irresistible, take the place of mere idle conjectures.

In the year 1605, Flurence Rivault, a gentleman of the bedchamber to Henri IV., and the preceptor of Louis XIII., discovered that an iron-ball, or bomb, with very thick walls, and filled with water, exploded sooner or later when thrown into the fire, if *its mouth were closed*, or, in other words, if you prevented the free escape of the steam as it was generated. The power of steam was here demonstrated by a precise proof, which, to a certain point, was susceptible of numerical appreciation,† whilst at the same time it revealed itself as a dreadful means of destruction.

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* Hero of Alexandria attributed those sounds, the objects of so much controversy, which the statue of Memnon produced when the rays of the rising sun darted upon it, to the passage, through certain openings, of a current of steam, which the heat of the sun was thought to have produced, at the expense of the liquid, which the Egyptian priests placed, it was said, in the interior of the pedestal of the Colossus. Solomon de Caus, Kircher, and others, have gone so far as to investigate the particular arrangements by means of which the priestly fraud was palmed upon the credulous. It appears evident, however, that their explanations are erroneous, if, indeed, there existed any thing of the sort requiring explanation.

† If some antiquarian think that I have not gone back far enough because I commence with Flurence Rivault, and if, according to the statement of

Upon this last-named fact, enlightened minds will not dwell with foreboding melancholy. They know well that mechanical powers, like human passions, will become useful or hurtful according as they are directed right or wrong. In the case of steam, it in fact requires only a very simple contrivance, to make available to productive labour, the formidable elastic power which, according to all appearance, shakes the earth to its centre, surrounds the art of the statuary with imminent danger, and breaks into a thousand pieces the strongest bombshell.

In what state do we find this projectile previous to its explosion? The lower part contains water at a very high temperature, *but still liquid*, and its remaining portion is filled with steam. This, according to the characteristic law of all gases, exercises its power equally in all directions; it presses with equal intensity upon the water, and the metallic sides which contain it. Let us now place a stopcock at the lower part of these sides,—on opening it, the water, forced by the steam, will issue forth with extreme velocity. If the stopcock was placed upon a tube, which, after taking a bend round the outside of the bomb, were then directed vertically from below upwards, the water would ascend in the tube in the ratio of the elasticity of the steam; or rather, for it is the same thing in other words, the water would rise according to the degree of temperature; and this ascending movement would find its limits only in the strength of the apparatus.

For this bomb let us now substitute a strong close boiler of large dimensions, and then there is nothing to prevent our forcing great masses of liquid to indefinite heights by the sole

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Alberti, who wrote in 1411, he should inform us that, in the beginning of the fifteenth century, lime-burners were much alarmed both for themselves and their kilns, on account of the explosions which were produced when the pieces of limestone had a cavity in their interior, I answer, that Alberti himself was ignorant of the real cause of these terrible explosions; that he attributed them to the transformation by flame of the air they contained *into steam*. To this I add, that the explosion of a piece of limestone thus accidentally hollow, supplied no means of that numerical calculation, of which the experiment of Rivault was evidently susceptible.

action of steam; and we shall have constructed, in every sense of the word, a steam-engine which might serve the purpose of drainage.

And now you have been made acquainted with that invention for which France and England have contended, as formerly the seven cities of Greece respectively claimed the honour of being the birthplace of Homer. On the other side of the Channel they have unanimously ascribed the honour of it to the Marquis of Worcester, of the illustrious house of Somerset. On this side, again, we maintain that it belongs to a humble engineer, almost forgotten by our biographers, namely, Solomon de Caus, who was born at Dieppe, or in its neighbourhood. Let us now cast an impartial glance upon the several claims of these two competitors.

Worcester, deeply implicated in the intrigues of the last years of the reign of the Stuarts, was shut up in the Tower of London. One day, according to the tradition, the lid of the pot in which his dinner was preparing was suddenly elevated. In prison what can one do but think? Worcester pondered upon the strange phenomenon which he had witnessed. The idea then suggested itself, that the same force which had raised the cover, might become, under certain circumstances, a useful and convenient motive power. On recovering his liberty, he published, in the year 1663, in a work entitled *The Century of Inventions*, the means by which he proposed to realize his expectations. These means, in all essential particulars, so far at least as they can be comprehended, are, the bomb half filled with liquid, and the ascending vertical tube which we have just described.

This bomb and this same tube are described in "*La raison des forces mouvantes*," the work of Solomon de Caus. There the idea is brought out distinctly, simply, and without any pretensions. There was nothing romantic in its origin, nor had it any connection either with the events of a civil war, or with a celebrated State-prison, or even with the sudden elevation of the pot-lid of an unfortunate prisoner; but, what is of far more importance in a question of priority, it is, according to the date of its publication, forty-eight years anterior to *The Century of Inventions*, and forty-one years before the imprisonment of Worcester.

Thus, brought to a comparison of dates, the controversy seems to be terminated; for who can maintain that the year 1615 did not precede the year 1663? But those whose principal aim seems to have been to remove every French name from this important chapter in the history of science, suddenly changed their ground as soon as *La raison des forces mouvantes* was resuscitated from the dusty shelves in which it had been long entombed. They, without hesitation, broke their former idol; the Marquis of Worcester was sacrificed to the desire of annihilating the claims of Solomon de Caus, and the bomb placed upon a burning furnace, with its ascending tube, ceased altogether to be the true germ of our present steam-engine!

For my own part, I cannot allow that that individual accomplished nothing that was useful, who, pondering upon the enormous power of steam, raised to a high temperature, was the first to perceive that it might serve to raise great masses of water to all imaginable heights. I cannot admit that no gratitude is due to that engineer who was the first also to describe a machine which was capable of realizing such effects. It ought never to be forgotten, that we can only then correctly judge of an invention, when we transport ourselves in thought to the time when it was proposed, and when we banish from our minds all the knowledge which has been accumulated during the ages posterior to its date. Let us suppose some ancient mechanist, Archimedes, for example, consulted upon the means of elevating water contained in a vast close metallic receiver. He would have suggested great levers, pulleys, simple and compound, the windlass, and probably his ingenious screw; but what would have been his surprise if, for the solution of the problem, some one had contented himself with a fagot and a match? Who, then, can refuse the title of an invention, to a contrivance at which the immortal author of the primary and true principles of statics and hydrostatics would have been astonished? This apparatus of Solomon de Caus, this close metallic vessel, in which is produced a moving power almost indefinite, by means simply of a fagot and match, will always maintain a distinguished place in the history of the steam-engine.*

It is exceedingly doubtful whether either Solomon de Caus or Worcester ever constructed the apparatus they proposed. This honour belongs to an Englishman, to Captain Savery. I have no hesitation in associating the machine which this engineer constructed in the year 1698, with those of his two predecessors, although it must be added he introduced some important modifications; and among others, that of generating the steam in a separate vessel. If it signify little as to principle, whether the motive steam be produced from the water which is to be raised, and in the interior of the same boiler in which it is about to act, or, whether it be produced in a distinct vessel, whence it is at will to be conveyed by means of a communicating pipe and a stopcock, to the surface of the liquid proposed to be raised, it is very different in a practical point of view. Another and a still more important change introduced by Captain Savery, will more appropriately find a place in the remarks we shall presently devote to the labours of Papin and Newcomen.

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* It has been published to the world, that J. B. Porta in the year 1606, in his *Spiritali*, nine or ten years before the publication of Solomon de Caus's work, gave a description of a machine, the operation of which was intended to elevate water by means of the elastic power of steam. I have elsewhere demonstrated that the learned Neapolitan *did not speak, either directly or indirectly, of any machine* in the passage alluded to; that his object—his only object—was to determine experimentally the relative volumes of water and steam; that in the small apparatus he employed for this purpose, the steam could not elevate the liquid, according to the author's own account, above a few inches; and that in the whole description of the experiment, there is not a single word that conveys the idea that Porta was aware of the power of this agent, or of the possibility of applying it to the production of a useful working machine.

Again, are there those who think that I ought to have named Porta on account simply of his researches concerning the transformation of water into steam? I answer, that the phenomena had been previously studied with attention by Professor Besson of Orleans, about the middle of the sixteenth century, and that one of the works of this mechanician contains, in 1569, an essay expressly upon the determination of the relative volumes of steam and water.

Savery entitled his work *The Miners' Friend;* but the miners seemed scarcely to appreciate the important compliment he paid them. With one solitary exception, none of them ordered his machines. They have only been employed in distributing water over the different parts of the palaces, of country houses, parks, and gardens, and they have not been used to raise water to a higher level than ten or fifteen yards. It ought also to be observed, that the danger of explosion would have been great, if the immense power had been employed, which their inventor contended might be reached.

Although the practical success of Savery was so far from being satisfactory, yet the name of this engineer should ever hold a very distinguished place in the history of the steam-engine. Individuals, whose whole lives are devoted to labours of a speculative character, are little aware how vast is the interval between a plan, however ably and perfectly formed, and its realization. Not that I allege with a celebrated German philosopher, that *Nature always exclaims No! No!* when we are about to raise any corner of the veil which covers her; but, prosecuting the metaphor, we may affirm, that the enterprise becomes so much the more difficult and delicate, that its success is so much the more doubtful, and that it requires both the concurrence of more numerous artists and the employment of a greater number of material elements. In all these respects, and in reference to the epoch in which he lived, no one was placed in more unfavourable circumstances than was Savery.

Hitherto we have spoken only of machines whose resemblance to the steam-engines of the present day may more or less be disputed. Now, however, we come to the consideration of the *Modern Steam-Engine*, which performs so important a part in our manufactories and steam-vessels, and is essential in almost every pit and mine. We shall see it commence, enlarge, and develop itself, at one time under the inspiration of some celebrated genius, and at another, under the mere spur of necessity; for "necessity is the mother of invention."

The first name which we encounter in this new period is that of Denis Papin. It is to Papin that France owes the honourable rank she may claim in the history of the steam-engine. The high satisfaction which his success inspires is not however without its alloy. The claims of our countryman are to be found only in foreign archives; he published his greatest works on the other side of the Rhine; his liberty was threatened by the Revocation of the Edict of Nantes; and it was in melancholy exile that he for a time enjoyed that blessing of which studious men are the most jealous, namely, tranquillity. Let us throw a veil upon these deplorable results of our civil discords,—let us forget that fanaticism attacked the religious opinions of the philosopher of Blois, and let us turn again to mechanics, in respect of which, at least, the orthodoxy of Papin has never been disputed.

There are in every machine two things to be considered: These are, first the moving power; and secondly the arrangement, more or less complicated, of the frame and moveable parts, by the aid of which the moving power transmits its action to the resistance. At the stage of perfection which mechanical science has now reached, the success of a machine intended to produce great effects, depends chiefly upon the nature of the moving power, on the manner of its application, and on the management of its force. Now, we find that it was to the production of an economical moving power, capable of effecting the unceasing and powerful strokes of the piston of a large cylinder, that Papin consecrated his life. The procuring afterwards from the strokes of the piston, the power requisite to turn the stones of a flour-mill, the rolls of a flatting-mill, the paddles of a steam-boat, the spindles of a cotton-mill; or to uplift the massy hammer, which with oft-repeated stroke thunders upon the enormous masses of red-hot iron just taken from the blast-furnace; to cut with great shears thick metal bars, as easily as you divide a ribband with your scissors; these, I repeat, are problems of a very secondary order, and which would not embarrass the most common engineer. Hence, therefore, we may occupy ourselves exclusively with the methods by means of which Papin proposed to produce his oscillatory movement.

Conceive a wide vertical cylinder open above, and reposing at its base upon a metal table pierced with a hole which a stopcock can at will shut and open. Let us now introduce into this cylinder a piston, that is to say a large and moveable circular plate, which accurately fits it. The atmosphere will press with all its weight upon the upper side of this kind of diaphragm, and will tend to push it from above downwards. That part of the atmosphere again which fills the lower part of the cylinder, will tend, by its reaction, to produce the inverse movement. This second force will be equal to the former if the stopcock be open, for gases press equally in all directions. The piston will thus find itself operated on by two opposing forces, which will produce an equilibrium. It will nevertheless descend, but only in virtue of its own gravity. A slight counterpoise somewhat heavier than the piston, will suffice to draw it contrariwise to the top of the cylinder, and to keep it there. Suppose the piston arrived at this point, we have now to seek for the means of making it *forcibly descend*, and then ascend again.

Suppose that, after having shut the lower stopcock, we should succeed in annihilating *suddenly* all the air contained in the cylinder,—in a word, in making a vacuum ; the piston receiving only the action of the external air, pressing from above, would *rapidly descend.* This movement accomplished, we might then open the stopcock, the air would then enter from beneath, and would counterbalance the pressure of the atmosphere above the piston. As at the commencement, the counterpoise would now raise the piston to the top of the cylinder, and every portion of the apparatus would be found in its original state. A second evacuation, or we may call it abstraction of the internal air, would make the piston again descend, and so on successively. The true moving power of this machine would here be the weight of the atmosphere. And let no one suppose that because we walk and even run with facility through the air, the atmosphere must therefore be very feeble as a moving power. With a cylinder of two metres in diameter the pressure of the piston in descending—the weight it might raise throughout the whole height of the cylinder at each stroke, would be about 600 cwt. This enormous power, frequently repeated, may be obtained by means of a very simple apparatus, provided we could discover a method, at once prompt and economical, whereby we might produce and destroy at pleasure an atmospheric pressure in a metallic cylinder.

This problem Papin resolved. His beautiful and grand solution consists in the substitution of an atmosphere of steam for the common atmosphere,—in the replacement of this latter by a vapour which, at the boiling point, has precisely the same elastic force, but with this important advantage, of which the common atmosphere is destitute, viz., that the power of aqueous vapour is enfeebled very rapidly when the temperature is lowered, and that it almost wholly disappears, if the refrigeration be carried sufficiently far. I shall, therefore, adequately characterize the discovery of Papin, and in a few words, by saying, that he proposed to make a vacuum in large spaces by means of steam, and that his method is at once prompt and economical.*

The machine in which our illustrious countryman was thus the first to combine the elastic force of steam with the property which steam possesses of being annihilated by cold, he never executed on a large scale. His experiments were always made on mere models. The water which was intended to produce the steam did not even occupy a distinct vessel: enclosed in the cylinder, it reposed upon the metallic plate which closed it beneath. This plate Papin heated directly, to transform the water into steam; and it was from the same plate he removed the fire when he wished to effect the condensation. Such a process, barely tolerable when experiment is intended to verify the accuracy of a principle, would evidently be altogether inadmissible, were the piston required to move with any degree of velocity. Papin remarked, that the end might be obtained " by different constructions which might readily be conceived," but left the constructions entirely unexplained. He devolved upon his successors both the merit of applying his pregnant conception, and that of discovering those details which alone can ensure the success of a machine.

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* An English engineer, deceived, no doubt, by an incorrect translation, asserted, not long ago, that the idea of employing steam in one and the same machine, as an elastic force, and as a rapid means of producing a vacuum, belonged to Hero. I have, however, proved, beyond dispute, that the mechanist of Alexandria never dreamed of steam; and that in his apparatus the alternate movement could have resulted only from the dilation and condensation of air proceeding from the intermittent action of the solar rays.

In our earlier researches concerning the employment of steam, we have had occasion to cite the ancient philosophers of Greece and Rome; one of the most celebrated mechanists of the school of Alexandria; a Pope; a courtier of Henry IV.; and an engineer of Normandy, that province so productive of great men, and which has ornamented our national galaxy of talent, with a Malherbe and a Corneille, a Poussin and a Fontenelle, with Laplace and Fresnel: we have also had to quote an English nobleman; a British engineer; and finally a French physician who was a member of the Royal Society of London, for we cannot but confess that Papin, almost always an exile, was nothing more than a corresponding member of our own Academy. Simple artisans and more humble workmen are now about to enter the lists; and so all classes of society will be found to have contributed their share to the production of a machine by which the whole world was to profit.

In the year 1705, fifteen years after the publication of Papin's first memoir in the Acts of Leipsic, Newcomen and Cawley, the one an ironmonger, and the other a glazier in Dartmouth, Devonshire, constructed (and mark, I do not say projected, which is a very different thing), I repeat, constructed a machine, which was meant to raise water from great depths, and in which there was a distinct vessel where the steam was generated. This machine, like the small model of Papin, consisted of a vertical metallic cylinder, shut at the bottom and open at the top, together with a piston, accurately fitted, and intended to traverse the whole length, both in ascending and descending. In the latter, as in the former apparatus also, when the steam was freely admitted into the lower part of the cylinder, so filling it, and counterbalancing the external atmospheric pressure, the ascending movement of the piston was effected by means of a counterpoise. Finally, in the English machine, in imitation of Papin's, so soon as the piston reached the limit of its ascending stroke, the steam which had impelled it was refrigerated; a vacuum was thus produced throughout the whole space it had traversed, and the external atmosphere immediately forced it to descend.

To produce the necessary cooling, Papin, as we have already stated, did nothing more than remove the brasier which heated the bottom of his small metallic cylinder. Newcomen and Cawley introduced a process greatly preferable in every respect. They caused a large quantity of cold water to flow freely in an annular space formed between the external wall of the cylinder of their machine, and a second cylinder, somewhat larger, with which they surrounded it. The cold communicated itself by degrees to the whole thickness of the metal, and finally reached the steam itself.*

Papin's machine, thus perfected in so far as it regarded the method of cooling the steam, or of condensing it, excited, in a high degree, the attention of mine proprietors. It was rapidly introduced into many counties of England, where it was of very considerable service. The want of rapidity in its movements, however—the necessary consequence of the slowness with which the vapour cooled, and so lost its elasticity, was, at the same time, the subject of great complaint. Accident happily indicated a very simple means of overcoming this inconvenience.

At the commencement of the eighteenth century, the art of boring great metallic cylinders, and of closing them hermetically by means of moveable pistons, was as yet in its infancy. Moreover, in the early machines of Newcomen, the piston was covered by a sheet of water, which was intended to fill the spaces included between the circular contour of this moveable portion and the internal surface of the cylinder. To the very great surprise of the makers, one of their machines one day commenced working with most unwonted speed. After numerous observations, it was ascertained that on that occasion the piston had been pierced, and that the cold water had found its way directly into the cylinder in small drops,—these, in traversing the steam, quickly condensed it. From this fortuitous observation is dated the complete suppression of the external refrigeration, and the adoption of an injection to *produce a shower of cold water* throughout the whole cylinder at the moment proper for the descent of the piston. The oscillations thus acquired all the velocity that could be desired.

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* Savery had previously had recourse to a current of cold water which he threw upon the external surface of a metallic vessel, thereby condensing the steam which the vessel contained. This, in fact, was the origin of his connection with Newcomen and Cawley; but it ought not to be forgotten, that the patent of Savery, his machines, and the work in which they are described, are all many years later then the memoirs of Papin

And once more we must remark that, on a different occasion, an accidental circumstance seems to have had a share in an improvement equally important. The first machine of Newcomen required the most unremitting attention on the part of the individual who unceasingly opened and closed certain stopcocks, first for the introduction of the steam into the cylinder, and then for injecting the cold shower for its condensation. It happened on one occasion that the person so employed was a boy named Henry Potter. His young companions at their sports, uttered cries of delight, which vexed him beyond endurance. He was all impatience to join in their sport, but his required duties did not allow him half a minute's absence. His anxiety excited his ingenuity, and led him to observe relations he had never before thought of. Of the two stopcocks, the one required to be opened at the moment that the beam (which Newcomen first and so usefully introduced into his machines) terminated the descending oscillation, and required to be closed precisely at the termination of the opposite one. The management of the other stopcock was precisely the reverse. The positions, then, of the beam, and of the stopcocks, had a necessary dependence upon each other. Potter seized upon this fact; he perceived that the beam might serve to impart to the other parts of the machine all the required movements; and on the spur of the moment he realized his conceptions. He attached a number of cords to the stopcocks; some to the one end of the handle, and some to the other, and these he attached to the most suitable parts of the beam, so that in ascending it pulled one set of the cords, and in descending the other, and so effectually, that all the work of his hand was entirely superseded. For the first time, the steam-engine went by itself; and now no other workman was seen near it but the fireman, who from time to time fed the furnace under the boiler.

For the cords of young Potter, the engineers soon substituted rigid vertical rods, which were fixed to the beam, and armed with small pegs which either pressed from above downwards, or from below upwards, as required; and thus turned the different stopcocks and valves. These rods themselves have since been replaced by other combinations; but, however humbling the avowal, all these expedients are nothing more than simple modifications of a contrivance suggested to a child by his desire to join in the gambols of his youthful companions.

There exist in the museums of the curious, a considerable number of machines from which industry had anticipated great things, but which the expense of working and keeping them in order has rendered little more than mere objects of curiosity. Such, in all probability, would have been the fate of Newcomen's machine, at least in those districts which were not rich in fuel, had not the labours of Watt, which I must now proceed to analyze, succeeded in conferring upon them an unlooked-for perfection. This perfection must not be considered as the result of some fortuitous observation, or of any single inspiration of genius: the author effected it by means of most assiduous labour, and by innumerable well conceived and very delicate experiments. We may well say that Watt took for his guide this celebrated maxim of Bacon, "To write, speak, meditate, or act when we are not provided with *facts* to direct our thoughts, is to navigate a coast full of dangers without a pilot, and to launch into the immensity of the ocean without either rudder or compass."

There was in the museum of the University of Glasgow, a small model of one of Newcomen's steam-engines which could scarcely ever be made to work satisfactorily. Dr Anderson, the Professor of Natural Philosophy, requested Mr Watt to repair it, and under the able hand of the artist all the defects of its construction disappeared, and from that time the apparatus annually performed its task in the class-room, to the astonishment of the admiring pupils. With this degree of success most men would have been satisfied. Not so Watt; who, according to custom, here beheld an occasion for the gravest study. His researches were successively directed to all the points which seemed capable of explaining the theory of the machine. He determined the extent to which the water dilated in passing from its liquid state into that of steam; he calculated the quantity of water which a given weight of coal could vaporize,—the quantity of steam, in weight, which each stroke of one of Newcomen's machine of known dimensions expended,—the quantity of cold water which required to be injected into the cylinder to give the descending stroke of the piston a certain force; and finally, the elasticity of steam at different temperatures. All these investigations would have occupied the lifetime of a laborious philosopher, whilst Watt brought all his numerous and difficult researches to a conclusion, without allowing them to interfere with the labours of his workshop. Dr Cleland wishing, not long ago, to shew me the house, near Port-Glasgow, whither our associate retired, after quitting the shop, to follow out his experiments, we found it pulled down. Our disappointment was great, but happily of short continuance, for, within the site on which it had stood, we found ten or a dozen vigorous workmen, who seemed occupied in doing all honour to the cradle of modern steam-engines; they were at work on boilers whose united dimensions certainly equalled those of the humble dwelling which then stood no more. In such a spot, and in such circumstances, the most elegant mansion, the most superb monument, or the most perfect statue could not have so much awakened the most interesting associations as these colossal caldrons!

If the properties of steam be still present in your recollection, you will at once perceive that the economical play of Newcomen's machine seemed to require two irreconcilable conditions. When the piston descends, the cylinder must be cold; if this be not the case, it encounters steam still highly elastic, which much retards its progress, and diminishes the effect of the atmospheric pressure. When, afterwards, steam at the temperature of 212° rushes into the same cylinder, if the parietes be cold, this steam in heating them is partially liquefied, and until the moment that their temperature likewise rises to 212°, the elasticity of the steam is decidedly diminished; hence slowness of movement must be the result; for the counterpoise does not raise the piston till there exist within the cylinder a force which is capable of counterbalancing the action of the atmosphere; and hence also increased expense; steam being very dear, as I have already explained. No doubt will be entertained of the great importance of this economical consideration, when I state that the Glasgow model at each stroke, used a volume of steam many times greater than that of the cylinder. The expense of the steam, or, which comes to the same thing, the expense of the combustible, or, in other words, the pecuniary cost indispensable for maintaining the action of the machine, would be many times less if we could succeed in doing away with the successive heatings and coolings, the inconveniences of which have just been described.

Watt in a very simple way resolved this apparently unsolvable problem. All he did was to add to the machine, as previously constructed, a vessel perfectly distinct from the cylinder, and communicating with it only by means of a narrow tube, supplied with a stopcock. This vessel, known by the name of *the condenser*, is the chief of Watt's inventions; and, in spite of all my desire to be short, I cannot but explain its mode of action.

If there exist a free communication between a cylinder filled with steam, and a vessel void of steam and air, the steam of the cylinder will pass in part and very rapidly into the vessel, and the current will not stop till the elasticity be the same throughout. Suppose, then, that with the help of an abundant and continual injection of water, the vessel be kept constantly cold throughout its whole capacity, the steam will be condensed there so soon as it enters;—the whole of the steam with which the cylinder was originally filled will thus come to be successively annihilated;—the cylinder will thus be freed of its steam without its sides being in the slightest degree cooled, and the new steam with which we may fill it, lose nothing of its elasticity.

The *condenser* entirely absorbs the steam of the cylinder, on the one hand, because it contains cold water; and on the other, because the rest of its capacity contains no elastic fluid. But so soon as the first condensation of the steam is effected, these two conditions of its efficiency have disappeared; the condensing water is heated by absorbing the latent caloric of the steam, and some vapour is formed from this hot water; the cold water also contained atmospheric air, which, of course, was disengaged in the process of heating. If, then, after every act of condensation, this hot water, and vapour and air, which the condenser contains, were not removed, it would become inefficient. Watt accomplished this triple evacuation by means of a common air-pump, where a piston was conveniently attached to the beam, and so was worked. The force required to keep the air-pump in action, diminishes by so much the power of the machine; but this forms but a small loss compared with what was sustained by the old method of condensing the steam, through the refrigerated sides of the cylinder.

One word more, and the advantages of another invention of Watt will become manifest to all. When the piston descends in Newcomen's machine, it is the atmosphere which pushes it. That atmosphere is cold, and must cool down the parietes of the metallic cylinder, open at top, along which it successively moves throughout their whole extent. These sides of the cylinder can recover from this refrigeration, during the ascending course of the piston, only at the expense of a certain quantity of steam. There exists no loss, however, of this sort, in the *modified engines* of Watt; the atmospheric action is wholly eliminated, and in the following manner. The cylinder is closed at the top, by a metallic cover, which has an opening nicely fitted, so that the cylindrical piston-rod moves freely in it, without allowing the slightest passage to air or steam. The piston thus divides the cylinder into two chambers which are distinct from each other and perfectly closed; when it is about to descend, the steam from the boiler readily reaches the upper chamber through a tube properly placed, and forces it downwards, as did the atmosphere in the machine of Newcomen. This movement meets with no obstacle, provided that, during the period of its execution, the lower part of the cylinder alone be in communication with the condenser, whither all the lower steam rushes and is liquefied. So soon as the piston has quite descended, the simple turning of a stopcock, makes the two parts of the cylinder situated above and below the piston communicate,*—these two spaces are filled with steam of the same degree of elasticity, the piston is then put in equilibrium, and reascends to the top of the cylinder, as in Newcomen's atmospheric machine, by the mere action of a counterpoise.

In prosecuting his researches as to the means of economizing steam, Watt reduced almost to nothing, the loss which resulted from the cooling down of the outside of the cylinder in which the piston played. For effecting this object, he enclosed the metallic cylinder in a wooden one somewhat larger, and filled the space between them with steam.

Thus, then, was the steam-engine completed. The improvements it received from Watt are evident, and of their immense utility, there cannot be a doubt. You will, therefore, anticipate, that it would immediately displace, as a draining apparatus, the comparatively ruinous machines of Newcomen. This, however, was far from being the case. The author of a discovery has always to contend with those whose interests may be affected, with the obstinate partisans of whatever is ancient, and finally, with those who are jealous,—and these three classes united form (must we confess it ?) the great majority of the public. Yet, to avoid a paradoxical result, I leave out of my calculation all those who had double motives. It is time alone that can disunite and scatter this phalanx of opponents. Nor will time alone do it. They must be energetically and unceasingly attacked; and the means used must be varied, as is done by the chemist whom experience has taught that the complete solution of certain compounds requires the successive employment of several acids. The strength of character and firm-ness of purpose, which, in the long run, defeat the most wily intrigues, are seldom united with an inventive genius. Watt, were it necessary, might be quoted as a convincing proof of the fact. His grand invention and most felicitous conception, that steam might be condensed in a vessel quite separated from the cylinder in which the mechanical action is going forward, was completed in the year 1765; and in two years, scarcely any progress was made to try its applicability upon the great scale. At length, however, his friends put him in communication with Dr Roebuck, the founder of the great establishment at Carron, so celebrated even at the present time. The engineer and the projector now associated themselves together, Watt yielding to him two-thirds of his patent. A machine was speedily constructed upon the new principles; it confirmed all the anticipations of theory; its success was complete, although, in the mean time, Dr Roebuck's fortune was injured. The invention of Watt would, without doubt, have repaired it; all that was required, was to apply to money lenders; but our brother associate thought it more simple to renounce his discovery, and to change his career. In the year 1767, when Mr Smeaton was executing the surveys and levellings between the rivers Forth and Clyde, preparatory to those gigantic works which were about to be executed in this part of Scotland, we find Watt conducting analogous operations along a rival line. Still later, he furnished a plan for the Monkland Canal, and executed it. Many projects of a similar kind, continued to occupy the attention of our associate till the close of the year 1773. Among these, we may mention the Crinan Canal, which was subsequently formed by Mr Rennie; also improvements in the harbours of Ayr, Port-Glasgow, and Greenock;* the building of bridges at Hamilton and Rutherglen; and the surveying the district of the celebrated Caledonian Canal, upon which he made a report, with plans and sections, subsequently referred to by Mr Telford. Without depreciating the importance of these labours I may still be permitted to add, that their interest was only of a local character; and that for their conception, superintendence, and conduct, there was no need of calling in the assistance of James Watt.

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* The communication with the condenser must at the same time be cut off.—Ed.

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* We may also mention the deepening the river Clyde; the improving the navigation of the Forth and Devon, and the Water of Leven; the making a canal from Machrihanish Bay to Campbeltown, and another between the grand canal and the harbour of Borrowstownness.—Edit.

Were I now for an instant to forget my duties as the organ of the Academy, and to aim at producing a smile rather than insisting upon important truths, the fact before us would supply the materials of a striking contrast. I might adduce not a few authors, who, at our weekly meetings, are wont to demand, with all their heart and might, leave to communicate the solitary remark, the trifling reflection, the hasty note which was conceived and written the previous evening. I might represent them cursing their destiny, when our laws, or the priority of another's communication, postponed their paper for a week; although they have the guarantee of the *paquet cacheté* being deposited in our archives. On the other hand we see the great inventor of a machine destined to constitute an epoch in the annals of the world, submit, without a murmur, to the stupid neglect of capitalists, and apply his superior genius for eight years, to the preparation of plans, to the making of surveys, to troublesome calculations of levelling, and to measurements of masonry. How strikingly does this exhibit the serene character, the subdued ambition, and the true modesty of Watt. But, indifference such as this, however noble its causes, was not devoid of blame. It is not without reason that society stamps with its reprobation, those who withdraw from circulation the gold hoarded in their coffers. And is that individual less culpable who deprives his country, his fellow-citizens, and the age in which he lives, of the treasures a thousand-fold more valuable, which are the products of the mind; who retains for himself alone those immortal discoveries, the sources of the noblest and purest delights of the soul; and who does not bestow on the artisan, mechanical contrivances which may indefinitely multiply the products of industry, which may diminish, to the profit of civilization and humanity, the effects of the inequalities of our lot, and which may ere long afford us the satisfaction of visiting the humblest dwellings, without discovering the heart-rending spectacle of fathers of families, and wretched children of both sexes, assimilated to the brutes, and hurrying prematurely to the tomb?

Towards the commencement of the year 1774 (after what we must call the indifference of Watt was overcome), he formed a connection with Mr Boulton of Soho, near Birmingham, a gentleman equally distinguished by his knowledge of the arts and his enterprising spirit.* The two friends applied to Parliament for prolongation of the privilege, for Mr Watt's patent was dated in the year 1769, and had only a few years to run. The introduction of the bill gave rise to an animated discussion. The celebrated mechanist thus writes to his aged father, in a letter, dated London, May 8. 1775. "After a series of violent and various opposition, I have at last got an act of Parliament. The affair has been attended with great expense and anxiety, and without many friends of great interest, I should never have been able to have carried it through, as many of the most powerful people in the

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* In the note with which he accompanied the last edition of Professor Robison's Essay on the Steam-engine, Watt expressed himself in the following terms concerning Mr Boulton. "Our friendship continued undiminished to the close of his life. As a memorial due to that friendship, I avail myself of this, probably my last, opportunity of stating, that to his friendly encouragement, his partiality for scientific improvements, and his ready application of them to the processes of art, to his intimate knowledge of business and manufactures, and to his extended views and liberal spirit of enterprise, must, in great measure, be ascribed whatever success has attended my exertions."

Mr Boulton's manufactory at Soho had existed for several years previous to the association spoken of in the text. This establishment, the first upon so great a scale which sprung up in England, is still quoted for the elegance of its architecture. There were here manufactured all kinds of first-rate articles in steel, plate-metal, silver, and or-molu, as also astronomical clocks, and painted glass. During the last twenty years of his life, Mr Boulton was occupied with improvements in the fabrication of the coinage. By the combination of some processes, which originated in France, with new presses, and an ingenious application of the steam-engine, he united an extraordinary rapidity with perfection of execution. Hence Mr Boulton, at the order of the English Government, recast all the copper-money of the United Kingdom. The economy and distinctness of this prodigious undertaking, rendered counterfeits almost impossible. The numerous executions with which the towns of London and Birmingham used annually to be distressed, ceased. It was on this occasion that Dr Darwin exclaimed in his "*Botanic Garden,*" "if a civic crown was given in Rome for preserving the life of one citizen, Mr Boulton should be covered with garlands of oak." Mr Boulton died in the year 1809, at the age of 81.

House of Commons opposed it." I was curious to learn to what class of society those members of Parliament belonged, to whom Mr Watt alludes, and who refused to the man of genius a small fraction of that wealth which he was about to create. Conceive my surprise, when I learned that at their head stood the celebrated Burke! Is it then the fact, that a man may be given to profound thought, may possess extensive knowledge and sterling honesty, be pre-eminently endowed with oratorical talents to move and carry along with him political assemblies, and yet be wanting in plain common sense? Since the important and wise improvements which Lord Brougham has introduced into the law of patents, inventors will not be subjected to that long series of annoyances to which Mr Watt was exposed.

So soon as Parliament renewed Mr Watt's patent, for a period of twenty-five years, he and Mr Boulton together, commenced at Soho, those establishments which have proved the most useful school of practical mechanics for the whole of England. They speedily commenced the construction of draining pumps of the largest dimensions, and repeated experiments demonstrated, that, in the production of equal effects, there was a reduction of three-fourths of the expense of the fuel consumed by those of Newcomen. From that moment the new engines spread over all the mining districts, and especially Cornwall. Boulton and Watt received, as remuneration, the third part of the value of the coal which was saved by the use of each of their machines; and we may judge of the commercial importance of the invention by the fact, that, in the single mine of Chacewater, where three pumps were employed, the proprietors thought it worth their while to purchase the rights of the inventors, at the price of L.2500 per annum for each engine. Thus, in a single establishment, the substitution of the *condenser*, effected in fuel alone, a reduction in expense of more than L.7500 per annum.

Men willingly consent to pay the rent of their dwelling or their farm; but this compliancy abandons them when they have to do with an idea, however great the profit or advantage it may have procured. Ideas! they exclaim, surely they cost no labour and no trouble. Who, besides, they add, can prove that in a very short time they would not have occurred to all the world? According to this reasoning, neither days nor months, nor years of priority, should confer the slightest privilege. These opinions, which there is now no occasion for my criticising here, had, from mere repetition, acquired a sort of prescriptive establishment. Men of genius, and the *manufacturers of ideas*, it seemed, ought to remain strangers to any thing like material enjoyments; and their history, forsooth, should continue to resemble the legends of the martyrs.

Whatever may be thought of notions like these, it is certain that the Cornish miners continued, with augmenting repugnance, to pay from year to year the gratuity they owed to the Soho engineers. They availed themselves of the first pretences which plagiarism afforded, to insist that their engagements were dissolved. The dispute was a serious one; it might have compromised Mr Watt's social position; and he therefore devoted much of his attention to it, and became quite a lawyer. The circumstances of the long and expensive processes which Boulton and Watt had to carry on, and which they ultimately gained, do not merit particular remark; but as we have just cited Burke among the opponents of the great mechanist, it seems just, on the other hand, to state that Roy, Mylne, Herschel, Deluc, Ramsden, Robison, Murdoch, Rennie, Cumming, More, and Southern, publicly and powerfully defended the rights of persecuted genius. It may not be useless to add, as a curious trait in the history of the human mind, that the advocates, who are sometimes accused of superfluity of words, reproached Watt, against whom they were leagued in great numbers, that he had invented nothing but ideas. This sentiment drew upon them, in open court, that chastisement of Rous, "Away, gentlemen! fret as you list against these untangible combinations, as you call Mr Watt's machines; but know that these pretended abstract ideas could crush you like so many gnats, and shoot you out of sight into the air."

The persecutions to which a man of sensibility is exposed, where the strictest justice would lead him to expect the unanimous expression of gratitude, rarely fail to discourage and sour his temper; and the amiability of Watt did not withstand the trial. Seven long years of litigation excited a feeling of vexation which sometimes manifested itself in bitter terms. "We have been so beset with plagiaries," he remarks, "that if I had not a very distinct recollection of my doing it, their impudent assertions would lead me to doubt whether I was the author of any improvement on the steam-engine; and the ill-will of those we have most essentially served, leads them to canvass whether such improvements have not been highly prejudicial to the commonwealth."

But, though much irritated, Watt did not suffer himself to be discouraged. At first, his machines, like those of Newcomen, were nothing more than simple pumps, in other words, simple means of raising water. In a few years, however, he transformed them into machines capable of producing all kinds of movements, and of indefinite power. His first step in this new career was the construction of the *double-acting engine.* To understand the principle of this engine, we must refer to the *modified machine*, already explained (p. 253). In it, as we have seen, the cylinder is closed; the external air is excluded; it is the pressure of steam, and not that of the atmosphere, which produces the descent of the piston; and finally, it is a simple counterpoise which produces the ascending movement, for at the moment the movement is effected, the steam being allowed to circulate freely between the upper part and the lower of the cylinder, presses the piston equally in the two opposite directions. Hence, it will be clearly perceived, that the *modified machine*, like that of Newcomen, has no real force except during the descending stroke of the piston.

A very simple change will remedy this serious defect, and present us with the *double-acting engine.* In the engine known under this name, as in that which we designated the Modified Engine, the steam of the boiler, when required, passes in freely above the piston, and depresses it without encountering any obstacle, because, at the moment, the lower part of the cylinder is in direct communication with the condenser. This movement once accomplished, and a certain stopcock being opened, the steam issuing from the boiler now rushes underneath the piston only, and elevates it, whilst the upper steam which had produced the descending movement, passes off to be liquefied in the condenser, with which it is now, in its turn, in free communication. The contrary movement of the stopcocks replaces all the different parts in their first state, so soon as the piston is at the summit of its elevation; and the same changes can be repeated indefinitely. The moving power here, it will be observed, is the steam exclusively; and the machine, with an exception dependent upon the inequality of the weight of the piston, has the same power whether the piston ascends or descends. Hence, from its first invention, it was justly designated the *Double-acting Engine.*

That he might render his new motive power of ready and convenient application, Mr Watt had to overcome additional difficulties. He had first to discover the means of establishing a *rigid communication* between the inflexible piston-rod, whose stroke was perpendicular, and the beam whose movement was circular. The solution which he gave of this important problem, is probably his most ingenious discovery.

Those who have seen a steam-engine at work, have probably been struck by the presence of a certain jointed parallelogram. At each double oscillation it opens and closes with the smoothness, and I had almost said the gracefulness, which so much charms us in the movements of a consummate actor. Follow attentively with your eye the progress of these different transformations, and you will find them subjected to the most curious geometrical conditions. You will perceive that *three* of the angles of the parallelogram describe in space certain arcs of circles, whilst *the fourth*—the angle which raises and depresses the piston-rod,—moves very nearly in a straight line. The vast utility of this result, astonishes mechanicians still less than the simplicity of the means by which Mr Watt obtained it.[*]

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[*] The following are the terms in which Mr Watt gave an account of his first trial of this jointed parallelogram:—"I have myself been much surprised with the regularity of its action. When I saw it in movement for the first time, it afforded me all the pleasure of a novelty, and I had quite the feeling as if I had been examining *the invention of another.*" Mr Smeaton, who was a great admirer of Mr Watt's inventive powers, conceived, nevertheless, that to confer *directly* the movements of rotation upon the axes, would never practically become either useful or economical. He maintained also, that steam-engines would never drain so effectually as by di-

Power, however, it should be observed, is not the only element of success in the labours of industry. Regularity of action is of scarcely less importance; and what degree of regularity is to be expected from a moving power which is procured from the fire, under the influence of the poker and shovel, and supplied by coals of very different qualities, under the influence, too, of workmen often far from intelligent, and almost always inattentive? We should expect that the propelling steam would be sometimes superabundant; that hence it would rush into the cylinder with the greater rapidity, so making the piston work more rapidly according as the fire was more powerful; and from such causes great inequalities of movement appear almost inevitable. But for all irregularities of this sort the genius of Watt provided a remedy. The valves through which the steam proceeds from the boiler to enter into the cylinder, have a variable area. When the speed of the machine is accelerated, these valves partially close, hence the steam enters less freely, and the acceleration is arrested: and, on the contrary, when the movement relaxes, then the apertures of the valves are increased. The mechanism which is necessary for effecting these different changes, connects the valves with an apparatus whose principle Mr Watt exhibited in the regulator of the sluices of certain flour-mills, and which he denominated the *governor*, but which is now commonly called the *regulator by centrifugal force.* Its efficacy is such, that a few years since there was to be seen at Manchester, in the cotton-mill of Mr Lees, a mechanist of great talent, a clock, which was set in motion by the steam-engine of the establishment, and which kept time, without any marked inferiority, with an ordinary clock of the common construction.

The regulator of Watt, and a skilful employment of fly-

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rectly pumping out the water: he believed that this liquid thus raised to the necessary height should then be thrown into the troughs, or poured upon the boards of common hydraulic wheels. The anticipations of Mr Smeaton have not in these respects been confirmed. Nevertheless, I saw, in the year 1834, in Mr Boulton's establishment at Soho, an old steam-engine, which is still employed in raising water from a large pond, and then throwing it upon the buckets of a large water-wheel, which was used when, in particularly dry seasons, the usual supply was not sufficient.

wheels, constitute the secret, the true secret, of the astonishing perfection of the manufactures of our epoch. It is this which now-a-days confers on the steam-engine a working movement which is wholly free from irregularity; hence it can as easily embroider muslin as forge anchors,—can weave the most delicate fabrics, as well as communicate a rapid movement to the ponderous stones of a flour-mill. This also explains how Mr Watt said, without fearing the reproach of exaggeration, that to avoid the intrusion of domestics, we might employ steam, and in cases of sickness could supply medicines through its silent agency. I am not ignorant that in popular estimation this gentleness of movement is supposed to be obtained at the expense of power. But this is an error, and a gross one; and the apophthegm "Much noise and little work," is not only true in the moral world,—it is also an axiom in mechanics.

A few words more, and we reach the termination of these technical details. Within these few years great advantage has been derived from not allowing a free communication between the boiler and cylinder, throughout the whole continuance of each stroke of the machine. This communication is accordingly interrupted when the piston has traversed, we shall say, a third of its course. The two remaining thirds of the stroke are thus accomplished in virtue of the pre-acquired velocity, and especially by the expansion of the steam. Now, Mr Watt had already indicated this procedure.* Excellent judges place it, in point of economical importance, on a level with the condenser. It appears certain that since its adoption the Cornish machines have yielded unlooked-for results; and that, with a bushel of coal, they realize the work of twenty men working for ten hours. Let us remember that in the coal districts a bushel of coal often does not cost *ninepence*, and it will be seen that Watt has established, that over a great part of England, a man's hard day's work—ten work hours to the day—may be done for less than a halfpenny.*

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* The principle of the expansion of steam, clearly indicated in a letter of Mr Watt's to Dr Small, dated in May 1769 (see the letter in Favey's Steam-Engine, vol. i. p. 339), was put in practice in the year 1776 at Soho, and in 1778 at the *Shadwell Water-Works,* from economical considerations. The invention, and the advantages which were expected from it, are fully described in the patent of 1782.

Such numerical valuations so strikingly prove the importance of the inventions of our learned associate, that I cannot resist the temptation of giving two other illustrations, both of which I borrow from Sir John Herschel, one of the most distinguished correspondents of the Academy. The ascent of Mont Blanc, starting from the valley of Chamouni, is very justly considered as one of the hardest tasks which it is possible for a man to execute in the course of two days. Thus the maximum of the labour which we are capable of undergoing in twice twenty-four hours, may be measured by the transport of the weight of our body to the height of Mont Blanc. This labour, or an equivalent to it, is executed by a steam-engine with the consumpt of two pounds of coals. It has thus been established by Watt, that the daily power of man does not exceed that which is contained in a pound of coal. Again, Herodotus informs us, that the construction of the Grand Pyramid of Egypt employed one hundred thousand men during the space of twenty years. The pyramid is formed of limestone; its volume can easily be calculated; and hence it is deduced that its weight is about thirteen millions of millions of pounds. To elevate this weight to the height of one hundred and twenty-five feet English, which is the height of the centre of gravity of the pyramid, it would be necessary to consume under the boiler of a steam-engine 630 chaldrons of coal; and I could name a foundery in Britain which consumes a greater quantity every week.

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* At a time when so many people are occupied with projects of rotatory steam-engines, it would be unpardonable were I not to state that Watt had not only thought of them (of which we find proof in his patents), but had actually constructed them. Mr Watt subsequently abandoned them, not because they did not work, but because they appeared to him decidedly inferior in an economical point of view to machines of double powers and rectilineal oscillations.

There are, in fact, few inventions, great or small, among those so admirably combined in our present steam-engines, which are not the development of some of the original ideas of Watt. Examine his labours, and, in addition to the principal points minutely enumerated in the text, you will find he proposed machines without condensation, in which, after having acted, the steam is dispersed in the air, and which were intended for localities where large quantities of cold water could not readily be procured. The operation of the principle of expansion in machines with several cylinders was also one of the projects of the Soho engineer. He suggested the idea of pistons, which should be perfectly steam-tight, although composed exclusively of metal. It was Watt also who first had recourse to mercurial manometers for measuring the elasticity of the steam in the boiler and the condenser, who conceived the idea of a simple and permanent gage by whose assistance might always be ascertained, with a glance of the eye, the level of the water in the boiler, and who, to prevent this level ever varying injuriously, connected the movements of the feeding pump with those of a float; and who, when required, placed in an opening in the cover of the principal cylinder of the machine the *indicator*, a small apparatus so constructed that it accurately exhibits the state of the steam, in relation to the position of the piston, &c. &c. Did time permit, I could shew that Watt was not less skilful and happy in his attempts to improve the boilers, to diminish the loss of heat, and to consume those torrents of black smoke which issue from common chimneys, however elevated they may be.

Copying Press—Heating by Steam—The Composition of Water—Bleaching by means of Chlorine—Experiments upon the Physiological Effects resulting from the Respiration of various Gases.

Birmingham, when Mr Watt went to establish himself at Soho, reckoned among the inhabitants of the neighbourhood, Priestley, whose name is universally known; Darwin, the celebrated author of the Zoonomia, and of a poem, "The loves of the Plants;" Withering, a physician and distinguished botanist; Keir, a chemist well known by the Notes of his translation of Macquer, and by an interesting memoir on the Crystallization of Glass; Galton, to whom we are indebted for an elementary treatise on Ornithology; and Edgeworth, the author of several esteemed works, and father of the celebrated Maria Edgeworth. These scientific men speedily became the friends of the distinguished mechanist, and most of them united in forming with him and Mr Boulton, an association under the name of "*The Lunar Society.*" This fantastic title has given rise to various mistakes; but it imported nothing more than that the night of meeting was that of the full moon, a time selected, in order that the members might comfortably reach their several domicils.

Every meeting of the Lunar Society gave fresh occasion, to remark the uncommon fertility of invention with which Watt was endowed. "I have thought," observed Dr Darwin on one occasion, "of a kind of double pen, a pen with two points, by which one might write the same thing twice over at the same time, and thus supply himself at once with the original and with a copy." "I hope," replied Watt, almost immediately, "to discover a better method for accomplishing the same object. I will mature my ideas to night, and communicate them to you to-morrow." The *Copying Press* was invented the next day; and even a small model was prepared, ready to shew its powers. This most useful instrument, now so generally adopted in all the offices and counting-rooms in England, has recently received some slight modifications, of which various artists have assumed the credit to themselves; but I can truly affirm, that the present form was described and delineated as early as the year 1780, in the patent of our associate.

Heating by means of Steam was an invention three years later in time, which Mr Watt introduced into his own dwelling in 1783. We have no wish here to conceal, that this ingenious method was previously described by Colonel Cooke, in the Philosophical Transactions for 1745;* but there it long lay overlooked and neglected. Mr Watt, however, had the sole merit of reviving it. He was the first to apply it; and it was his calculations upon the extent of surface necessary to be heated in rooms and edifices of different sizes, which served at the first as the basis of the plans of most English engineers.

Had Watt, during his long career, done nothing more than introduce the separate condenser, the working steam expansively, and the jointed parallelogram, he would have occupied one of the first places among the small number of individuals whose life constitutes an epoch in the annals of the world. It appears to me, however, that his name is also connected in a distinguished manner with the greatest and most prolific discovery of modern chemistry, namely, the *discovery of the composition of water.* My assertion to many may appear rash, inasmuch as in the numerous works which have professedly treated upon this capital point in the history of the sciences, Watt has been forgotten. I trust, notwithstanding, that you will be ready to follow my discussion without prejudice; that you will not be diverted from the inquiry by authorities who have taken the other side of the question, and who, after all, are less numerous than is generally supposed; that you will remember how few authors now-a-days go direct to the original sources, how little disposed they are to encounter the musty dust of our libraries, and, on the contrary, how much easier it seems to them to live upon the erudition of another, reducing the composition of a book to the simple labour of compilation. The warrant which I hold for your confidence seems to me more serious; I have examined numerous published memoirs, and the whole of a very voluminous authentic correspondence still in manuscript; and if I now come, after a lapse of fifty years, to claim for James Watt an honour which was too readily conceded to one of his most illustrious countrymen, it is because I consider it useful to demonstrate, that, within the walls of our scientific associations, truth is sooner or later brought to light, and that, in the matter of discovery at least, there is no prescription.

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* By the work of Mr Robert Stuart, I find that Sir Hugh Platte, previous to Colonel Cooke, had foreseen the possibility of applying steam to the heating of apartments. In the *Garden of Eden* of this author, published in the year 1660, we find mention made of something of an analogous kind for preserving plants during winter in greenhouses. Sir Hugh Platte proposed to place covers of tin, or of some other metal, upon the vessels that were used for cooking, and thus to connect pipes to openings in the covers of these vessels, by which the heated vapour might be carried wherever it was desired.

The four pretended elements of fire, air, earth, and water, whose various combinations were supposed to produce all known bodies, constitute one of the legacies of a brilliant philosophy, which dazzled and misled the most noble intelligences. Van Helmont was the first who shook, though but slightly, one of the principles of this ancient theory, by calling the attention of chemists to several permanent elastic fluids or *airs*, which he called *gases*, whose properties differed from those of common air, or elemental air. The observations of Boyle and of Hooke created still graver difficulties, for they established that the common air, necessary to respiration and combustion, undergoes very remarkable changes in the course of these processes, and such changes in its properties, as to imply that it is a compound body. The numerous observations of Hales; the successive discoveries of carbonic acid gas by Black, and of hydrogen by Cavendish; of nitrous acid, of oxygen, of muriatic and sulphurous acids, and of ammonia by Priestley, gave the finishing blow to the antiquated notion that air was simple and elemental, and dismissed it as one of the rash and almost always false conceptions, which proceed from those who have the hardihood to consider themselves called not to discover, but to divine, the footsteps of Nature.

In the midst of these remarkable discoveries, water had always maintained its character of an element. The year 1776, however, was distinguished by one of those observations which necessarily led to the overturn of this general belief; whilst, at the same time, we must avow, that from the same period are to be dated those strange attempts which chemists for a long time made to resist the consequences which naturally flowed from their experiments. The observation to which I here particularly allude, was one of Macquer's. This judicious chemist having placed a white porcelain saucer over the flame of some hydrogen gas, which burned steadily as it issued from a bottle's mouth, observed that the flame was not accompanied by any smoke properly so called, and that it deposited no soot. The portion of the saucer upon which the flame struck—or, to use his own words, which it *lapped*—was soon covered with very conspicuous drops of a liquid similar to water, and which, upon trial, was found to be pure water. Here, assuredly, was a singular result, well worthy of attention: it was in the midst of flame, in that portion of the saucer, as Macquer said, lapped by the flame, that watery drops were deposited! This chemist, however, did not seize upon the fact;—he was in no degree astonished at what was so wonderful; he simply states it, without the slighest comment; and failed to perceive that he had touched upon the very threshold of a grand discovery.

In the sciences of observation, then, is genius to be reduced to the mere capability of saying at the fitting moment,—*Why?*

In the physical world, we consider as volcanos, mountains which never have had more than a single eruption; and in the intellectual world, in like manner, there are men who, after one flash of genius, for ever disappeared from the history of science. Such an one was Warltire, whose truly remarkable experiments fall here in chronological order to be cited. At the beginning of the year 1781, this philosopher conceived that an electric spark could not traverse certain gaseous mixtures without inducing a change in them. An idea so novel, which no previous analogy could have suggested, and of which such wondrous applications have since been made, in my apprehension, requires that all the historians of the science should render due honour to its author. Happily for himself, he foresaw that an explosion would be a necessary consequence; and hence he made his first experiment in a metallic vessel in which he had put a mixture of common air and hydrogen.

Cavendish speedily repeated the experiments of Warltire. The *certain date* (and by this term I mean such an one as results from a paper having been given in, a communication having been read in a scientific society, or a treatise having been printed), the *certain date*, I repeat, of this investigation is previous to the month of April 1783; because Priestley quotes the observations of Cavendish in a memoir dated the 21st of that month. The citation, it should be noted, informs us only of a single particular, viz. that Cavendish had obtained *water* by the detonation of a mixture of oxygen and hydrogen, a result which Warltire had previously obtained.

In his memoir of the month of April, Priestley added an important circumstance to those resulting from the experiments of his predecessors: he proved that the weight of the water which is deposited upon the sides of the vessel, at the instant of the detonation of the oxygen and hydrogen, is precisely the same as the weights of the two gases.

Watt, to whom Priestley communicated this important result, immediately perceived, with the penetration of a superior man, that a proof was here afforded that water was not a simple body. "What," he writes to his illustrious friend, "are the products of your experiment? They are *water*, *light*, and *heat*. Are we not, thence, authorized to conclude that water is a compound of the two gases, oxygen and hydrogen, deprived of a portion of their latent or elementary heat; that oxygen is water deprived of its hydrogen, but still united to its latent heat and light? If light be only a modification of heat, or a simple circumstance of its manifestation, or a component part of hydrogen, oxygen gas will be water deprived of its hydrogen, but combined with latent heat."

This passage, so clear, so precise, and logical, is taken from a letter of Watt's, dated 26th April 1783. The letter was communicated by Priestley to several of the scientific men in London, and was transmitted immediately afterwards to Sir Joseph Banks, the President of the Royal Society, to be read at one of the meetings of that learned body. Circumstances, which I suppress as being foreign to the present discussion, retarded the reading of the letter for about a year, but it remained the while in the archives of the Society. It appears in the 74th volume of the Transactions, with its true date, April 26. 1783. It is there to be found embodied in a letter from Watt to Deluc, bearing date 26th of November 1783, and is distinguished by inverted commas, supplied by the secretary of the Royal Society. I ask not indulgence for this profusion of details, because it is clear that the minute comparison of dates can alone bring the truth to light concerning a discovery which confers the highest honour upon the human intellect.

Among those who put in their claims to be the authors of this most pregnant discovery, we shall presently see appearing the two greatest chemists of whom France and England can boast. As every one will anticipate, I speak of Lavoisier and Cavendish. The date of the public reading of the memoir in which Lavoisier gives an account of his experiments, whereby he explained his views upon the production of water by the combustion of oxygen and hydrogen, is later by two months than the deposit of Watt's letter already alluded to, in the archives of the Royal Society of London.

The celebrated memoir of Cavendish, entitled "Experiments upon Air," is still later, being read on the 15th of January 1784. It could not fail to be a matter of astonishment, that facts so well authenticated should become the subject of an animated controversy, were I not immediately to bring under notice a circumstance to which I have not hitherto alluded.

Lavoisier distinctly states, in precise terms, that Mr Blagden, secretary of the Royal Society of London, was present at his first experiments on the 24th June 1783; and "that he told him that Cavendish, having previously endeavoured, in London, to burn hydrogen in close vessels, had obtained a sensible quantity of water." Cavendish, also, in his own memoir, alludes to the communication made by Blagden to Lavoisier. According to him, it was more ample than the French chemist avows; and he states that the confidential communication embraced the conclusions to which the experiments led; in other words, the theory of the composition of water. Blagden also himself took part in the controversy; and in Crell's Journal, in the year 1786, did what he could to confirm the assertion of Cavendish. According to him, the experiments of the Parisian academician were only a simple verification of those of the English chemist; and he assures us, that he announced to Lavoisier, that the water produced in London was of a weight precisely equal to that of the two consumed gases. Finally, Blagden adds: *Lavoisier has said the truth, but not the whole truth.* This reproach is severe; but, if it was deserved, shall I not materially diminish its severity, if I prove that, with the exception of Watt, all those whose names appear in this piece of history were more or less exposed to it?

Priestley reports in detail, and as his own, the experiments from which it results that the water produced by the detonation of a mixture of oxygen and hydrogen, is of a weight precisely equal to that of the two consumed gases. Cavendish, some time after, claims this conclusion as his property, and insinuates, that he had communicated it verbally to the chemist of Birmingham.

From this equality of weights, Cavendish deduces the consequence that water is not a simple body. In the first instance, he makes no mention whatever of a memoir deposited in the archives of the Royal Society, and in which Watt developed the same theory. It is true that, when Cavendish's paper was printed, Watt's name is not omitted; but it is not in the records that the account of the labours of the celebrated engineer had been found: it is stated that the information was obtained from a paper recently read at a public meeting. It is however now clearly established, that the paper referred to, was read many months subsequent to the memoir in which Cavendish alludes to it.

Appearing upon this field of controversy, Blagden announces his firm determination to clear up and settle every thing. He does not flinch from any accusation, or from the citation of any date, so long as his object is to insure for his protector and friend Cavendish, the priority in reference to the French chemist. So soon, however, as he takes up the question as between his own two countrymen, his explanations become altogether vague and obscure. "During the spring," he remarks, "of the year 1783, Mr Cavendish shewed us that he had necessarily deduced from his experiments, the conclusion, that oxygen is nothing else than water deprived of its phlogiston (that is to say, its hydrogen). *About the same time*, the news reached London, that Mr Watt of Birmingham had been led by some observations to a similar opinion." The expression, *About the same time*, to adopt Mr Blagden's own phrase, was not *the whole truth*. *About the same time decides nothing.* Questions of priority may depend upon months, weeks, days, or even minutes. To be clear and precise, as he had promised, he ought to have told us if the verbal communication made by Cavendish to several members of the Royal Society, preceded, or followed, the reception of the news of Watt's opinion. Is it conceivable that Blagden would have failed to be explicit upon a fact of such importance, if he could have cited an authentic date in favour of his friend?

To make the confusion complete, even the compositors, printers, and correctors of the press of the Philosophical Transactions would appear to have taken part in the dispute. Many of the dates are there inaccurately given. In the separate copies of his memoir, which Cavendish himself distributed, I perceive a mistake of a whole year. By a sad fatality, for it is a real misfortune involuntarily to give occasion to grievous and unmerited suspicions, not one of these typographical errors was favourable to Watt! Let it not be supposed that, by these remarks, I mean to inculpate the literary honesty of the celebrated men whose names I have mentioned; they prove merely, that where matters of discovery are concerned, the narrowest justice is all that one can expect from a rival or competitor, however high may have been his previous reputation. Cavendish would scarcely listen to his men of business, when they came to consult him concerning the disposal of his prodigious wealth; but we may perceive, he had not the same indifference concerning his scientific property. We ought to insist, therefore, in demanding, after the example of the judges in courts of law, that the historians of science should never receive, as valid titles of property, any other than written, I should perhaps add published, titles. It is then, and then only, that an end will be put to those constantly recurring disputes which are usually agitated at the expense of national vanity; and it is then only that the name of Watt will assume that distinguished place in the history of chemistry to which it is justly entitled.

The solution of a question of priority, where, as in the present instance, it is grounded upon the most attentive examination of printed memoirs, and a minute comparison of dates, possesses all the characters of a complete demonstration. Nevertheless, it may not be superfluous to notice slightly, various difficulties to which respectable individuals have seemed to me to attach some importance.

How, it has been said, can it be admitted that, amidst the immense turmoil of commercial business,—engaged with a multitude of law-suits, and obliged to provide, by the ingenuity of every passing day, for the difficulties of an infant discovery, Watt could have found time to follow, step by step, the progress of chemistry, to originate experiments, and propose explanations which even the masters of the science had not foreseen? To this difficulty, I make a very short and conclusive answer. I have now in my possession the copy of an active correspondence, relating principally to chemistry, which Watt maintained during the years 1782-3, and 4, with Priestley, Black, Deluc, the engineer Smeaton, Gilbert Hamilton of Glasgow, and Fry of Bristol.

Another objection, proceeding from a profound knowledge of the human heart, appears more specious. Since the discovery of the composition of water is one that ranks at least as high as the admirable inventions combined in the steam-engine, can it be supposed that Watt consented with satisfaction, or without even testifying his displeasure, to see himself despoiled of the honour which it would for ever have conferred on his name ? The only defect of this reasoning is, that it has not the shadow of foundation. Watt never renounced the part which legitimately belonged to him in the discovery of the composition of water. He caused his paper to be printed with scrupulous accuracy in the Philosophical Transactions. A detailed note determined authoritatively the dates when the several parts of this paper were presented. What more could, or ought, a philosopher of Mr Watt's character to have done, except to wait patiently for the time when justice would be awarded. Besides, the imprudence of Deluc had almost forced our associate from his usual equanimity. The Genevese philosopher, after having advertised the celebrated engineer of the inexplicable omission of his name in the first impression of Cavendish's memoir; and, after having characterized this neglect in terms which the celebrity of the parties does not permit me to repeat, writes to his friend : "I would almost counsel you, in your circumstances, to extract from your discoveries, practical results which will improve your fortune. You must avoid causing people to be jealous." These words wounded Watt's noble mind. "As to what you say, "he replies," about making for myself *des jaloux*, that would weigh little with me, for were I convinced I had had foul play, if I did not assert my right, it would be from a contempt for the modicum of reputation which would result from such a theory, from a conviction of my own mind I was their superior, or from an indolence that makes it more easy for me to suffer wrongs, than to seek redress. In point of interest, so far as connected with money, that would be no bar, for though I am dependent on the favour of the public, I am not on Mr Cavendish or his friends."

Few, I apprehend, will consider that I have attached too much importance to the theory which Watt suggested in explanation of Priestley's experiments. Those who refuse to pay a just tribute to this theory, because it now appears to be nothing more than the inevitable consequence of the facts, forget that the most beautiful discoveries of the human intellect have been the most distinguished for their simplicity. What did

Newton himself, when repeating an experiment which had been known for fifteen centuries, he discovered the composition of white light? He attached to that experiment an interpretation so natural, that now-a-days, it seems impossible to find any other. All that you obtain, says he, with the help of any process whatsoever, from a pencil of white light, was contained therein in its state of mixture. The glass prism has no creative power. If the parallel and infinitely slender pencil of solar light which strikes upon the one face issues from the other divergingly, and with increased breadth, it is because the glass separates that which, in the white ray, was by its nature, unequally refrangible. These words are nothing more than a literal interpretation of the well known experiment of the prismatic solar spectrum, an interpretation, however, which had escaped the penetration of Aristotle, Descartes, and Robert Hooke.

But, without departing from our present subject, let us come to arguments which bear still more directly on this point. The theory conceived by Watt concerning the composition of water reaches London. If, according to the apprehension of the time, it was as simple and as evident as it now appears to be, the Council of the Royal Society would not have failed to adopt it. But it was far from doing so; its *strangeness* made them even doubt the truth of Priestley's experiments: *they even laughed at it*, says Deluc, *as at the explanation of the dent d'or.* Again, a theory, the conception of which was attended with no difficulty would certainly have been disdained by Cavendish; and yet, with what pertinacity, under the influence of that ingenious man, did Blagden claim priority of discovery in opposition to Lavoisier. Priestley, upon whom a considerable share of the honour attached to the discovery of Watt must naturally fall, and whose affectionate regard for the celebrated engineer cannot be questioned, wrote to him, under date of 29th April 1783: "You will examine with surprise and indignation the sketch of an apparatus with which *I have undermined to its very basis your beautiful hypothesis.*"

Upon the whole, then, a theory which was ridiculed at the Royal Society,—which forced Cavendish out of his habitual reserve, and which Priestley, regardless of the fresh honour it brought him, set himself deliberately to overturn,—deserves to be recorded in the history of science as a great discovery, whatever notion our present intimacy with the subject might lead us to adopt concerning it.*

Bleaching by means of Chlorine, the beautiful invention of Berthollet, was introduced into Britain by James Watt, soon after having paid a visit to Paris, towards the end of the year 1786.† He constructed all the necessary apparatus, directed their suitable arrangement, superintended the first trials, and then confided to his father-in-law Mr Macgregor, the general introduction of the valuable improvement. In spite of all the urgent solicitations of the illustrious engineer, our celebrated countryman obstinately refused (however extraordinary it may appear in the age in which we live) to be associated in an undertaking which, to him, was free from risk, and of which, it appeared, the profits must be very great.

No sooner, in the latter half of the last century, were the numerous gases discovered, and the important agency they exerted in the explanation of chemical phenomena, than the question of their employment as powerful medicines was canvassed and advocated. Dr Beddoes especially prosecuted this subject with sagacity and perseverance. With the help of private subscriptions, he was enabled to establish at Clifton, near Bristol, an institution, known under the name of the *Pneumatic Institution*, where the remedial properties of the gases generally might be carefully studied. This establishment, for a time, was more immediately superintended by Humphry Davy, then a young man just beginning his brilliant career. It was also honoured with the name of Watt as one of its founders; and the celebrated engineer did more, for he planned, described, and constructed, in the manufactory of Soho, the apparatus which was employed in preparing the different gases, and in administering them to the patients. I find that many editions of his Directions were required, and were published in the years 1794, 1795, and 1796.

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* Lord Brougham was present at the public meeting of the Institute, when, in the name of the Academy of Sciences, I paid this tribute of gratitude and admiration to the memory of Watt. On his return to England, he collected some valuable documents, and studied anew the historical question to which I have devoted so much space in this memoir. He investigated the subject, in his usual masterly style, and with that scrupulous care, in some degree judicial, which might be expected from the former Lord Chancellor of Great Britain. I owe it to an act of kindness which I duly appreciate, that I am able to present to the public the result of the labours of my illustrious fellow member. (See page 316 of the present No.)

† This journey was undertaken with Mr Boulton, at the instance of the French Ministry, to advise them respecting the substitution of steam for water, as the moving power in the water-works at Marly. The state of the finances, and the approaching revolution, put an end to the plan at the time.—Edit.

The thoughts of our associate were directed to this interesting subject, from the circumstance that many of his connections and friends were cut down in early life, by diseases of the chest; and he imagined that it was in this class of complaints that the specific properties of the new gases would be most strikingly manifested. He also anticipated some benefit would be derived from the iron and zinc, which hydrogen contains in impalpable molecules, when prepared by certain processes. I shall add, in conclusion, that among the numerous notes supplied by physicians, and published by Dr Beddoes, announcing results more or less favourable, there is one, signed John Carmichael, respecting the complete cure of one of his servants, named Richard Newberry, to whom Watt had himself administered, from time to time, by way of respiration, a mixture of steam and carbonic acid. Though perfectly aware of my incapacity to come to any satisfactory conclusion on a matter of this sort, I must take the liberty of expressing my regret that a plan of treatment which numbered Watt and Jenner among its adherents, should be at present entirely abandoned, and this without there being adduced any connected series of experiments in manifest opposition to those of *the Clifton Pneumatic Institution.**

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* Twenty years previous to the existence of the Pneumatic Institution, Watt had applied his chemical and mineralogical knowledge to the improvement of a pottery which, along with some friends, he had established at Glasgow, and of which he continued a partner to the close of his life.

Watt in Retirement—Details respecting his Life and Character—His Death—The numerous Statues erected to his Memory—Reflections.

In the year 1764, Mr Watt had married his cousin Miss Miller. She was an accomplished person; and her wit, imperturbable sweetness, and cheerfulness of disposition, speedily rescued the celebrated engineer from an oppression of lassitude, discouragement, and misanthropy, which a nervous attack, and the injustice which he experienced, had well nigh rendered fatal. Without the cheering influence of his wife, Watt perhaps would never have published to the world his beautiful inventions. Of this marriage were born four children, two sons and two daughters. At an after period Mrs Watt expired in childbed, and her infant did not survive her. Her husband at the time was absent, engaged in the north of Scotland, with the plans of the Caledonian Canal. I shall here take the liberty of transcribing, in all their native simplicity, a few lines from the journal in which he was in the habit of recording daily his most private thoughts, his fears and his hopes. "I did what I could to force grief from my mind; but feared to come home where I had lost my kind welcomer. In her I lost the comfort of my life, a dear friend, and a faithful wife." Here is a striking picture of heartfelt sorrow, which may serve to shake the confidence of those system-makers, who, despite of innumerable instances to the contrary, deny the free and kindly play of the feelings to men whose intelligence finds its nourishment amid the sublime and imperishable truths of the exact sciences. After several years of widowhood, Mr Watt had the happiness to find in Miss Macgregor, a companion, rendered worthy of him, by the variety of her talents, the soundness of her judgment, and the strength of her character.*

On the expiration of the term to which his patent had been extended by Parliament, viz. in the beginning of the year 1800, Mr Watt withdrew himself entirely from business. His two sons succeeded him; and, under the enlightened direction of the younger Mr Boulton, and the young Messrs Watt, the establishment at Soho continued to prosper, and became more extended than ever. Even to the present day, it occupies the first rank among the English manufactories of great and powerful machines. Mr Watt's second son, Gregory Watt, had distinguished himself at an early period of his short career, both by his literary labours, and by some geological investigations; but he was cut off, by a disease of the chest, in the year 1804, at the age of twenty-seven. This afflictive event overwhelmed the illustrious engineer, and it required the most anxious attentions of his family and friends to supply some balm to a heart that was well nigh broken. This deep grief has been assigned as the reason of the almost total silence which Mr Watt maintained during the latter years of his life; and I am far from denying it had its share in the result. There is not, however, any occasion for resorting to extraordinary causes, when we reflect upon what were the inherent inclinations of his mind. So far back as the year 1783, we find him stating in a letter to his friend Dr Black, "I wish you to be quite aware that I have no desire to occupy the public with the experiments I have made;" and elsewhere we find these somewhat extraordinary words in the mouth of a man who has filled the world with his renown, "I know but two pleasures—idleness and sleep." That sleep, however, was light; and the slightest excitement roused him from his favourite indolence. Every object presented to his notice, gradually received, in the machinery of his mind, changes of form, of construction, and of nature, which rendered them susceptible of important applications; and when no occasion offered of realising these conceptions, they were lost to the world. An anecdote will explain my meaning.

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* This lady deceased in the year 1832, at a very advanced age. She had misfortune of surviving the only two children she ever had.

A water-company in Glasgow had established, on the right bank of the river Clyde, great buildings, and powerful machines, for the purpose of conveying water into every house in the town. When the works were completed, it was discovered that on the other side of the river there was a spring or rather a kind of natural filter, which abundantly supplied water of a very superior quality. To remove the works was now out of the question; but a question arose as to the practicability of drawing the water from wells on the left bank, by means of the pumping-engines then existing on the right bank, and through a main-pipe to be carried by some means across the river. In this emergency Watt was consulted; and he was ready with a solution of the difficulty; pointing to a lobster on the table, he shewed in what manner a mechanist might, with iron, construct a jointed tube which would be endowed with all the mobility of the tail of the crustacea; he accordingly proposed a complete jointed conduit-pipe, capable of bending and applying itself to all the inflections, present and future, of the bed of a great river;—in fact, a lobster-tail of iron, two feet in diameter, and a thousand feet in length. He soon after furnished plans in detail, and drawings; and the design was executed for the Glasgow Water Company, with the most complete success.*

Those who were favoured with the personal acquaintance of our honoured associate, have not hesitated to declare, that in him the invaluable qualities of the heart were even superior to those of his head. His singular candour, his child-like simplicity of manners, his most scrupulous love of justice, and his inexhaustible benevolence, have left in Scotland, and throughout Britain, recollections which will never be effaced. But with all this disposition, so moderate and gentle, Mr Watt writhed when he heard an invention ascribed to any other than the true author, and especially when some base flatterer would attempt to enrich him, at the expense of another. In his eyes, scientific discoveries were property of the highest order; and whole hours of discussion he considered well spent, if he succeeded in doing justice to modest men, whose inventions had been filched from them by plagiarists, or who were merely overlooked by an ungrateful public.

The memory of Watt might be cited as prodigious, even in comparison of all the wonders which have been narrated of this faculty as possessed by a few privileged individuals. Its extent, however, was its least merit; for while it made him master of every thing that was of real value, it wholly rejected, and almost instinctively, whatever was superfluous, and not worth the keeping.

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* We have thought it necessary to alter somewhat the version of this account given in the original, as M. Arago seems to have misapprehended the facts which were stated to him regarding the Glasgow Water Company. A full account of the flexible Water-Main, by Sir John Robison, together with an illustrative plate, will be found in the *Edinburgh Philosophical Journal*, vol. iii. 60.—Edit.

The variety of the knowledge of our associate, would have been absolutely incredible, were it not attested by many eminent men. Lord Jeffrey, in his eloquent notice, very happily characterizes the understanding of his friend, at once strong and delicate, by comparing it to the proboscis of the elephant, which serves with equal facility to lift a straw, or uproot the oak.

The following are the terms in which Sir Walter Scott speaks of his illustrious countryman in the preface to The Monastery. "It was only once my fortune to meet Watt, when there were assembled about half a score of our northern lights.* Amidst this company stood Mr Watt, the man whose genius discovered the means of multiplying our national resources to a degree, perhaps, even beyond his own stupendous powers of calculation and combination; bringing the treasures of the abyss to the summit of the earth,—giving to the feeble arm of man the momentum of an Afrite,—commanding manufactures to arise,—affording means of dispensing with that time and tide which wait for no man,—and of sailing without that wind which defied the commands and threats of Xerxes himself. This potent commander of the elements,—this abridger of time and space,—this magician, whose cloudy machinery has produced a change in the world, the effects of which, extraordinary as they are, are perhaps only beginning to be felt,—was not only the most profound man of science, the most successful combiner of powers, and calculator of numbers, as adapted to practical purposes,—was not only one of the most generally well-informed, but one of the best and kindest of human beings. There he stood, surrounded by the little band of northern literati. Methinks I yet see and hear what I shall never see or hear again. In his eighty-first year, the alert, kind, benevolent old man, had his attention at every one's question, his information at every one's command. His talents and fancy overflowed on every subject. One gentleman was a deep philologist,—he talked with him on the origin of the alphabet, as if he had been coeval with Cadmus; another a celebrated critic,—you would have said that the old man had studied political economy and belles-lettres all his life;—of science it is unnecessary to speak, it was his own distinguished walk. And yet when he spoke with your countryman, you would have supposed he had been coeval with Clavers and Burley,—with the persecutors and persecuted; and could number every shot that the dragoons had fired at the fugitive Covenanters."

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* At the table of one of the Commissioners of Northern Lighthouses.—Edit.

Had our associate been at all solicitous, he might easily have acquired a name among the writers of Romance. In the circle of his more intimate acquaintances, he seldom failed to improve upon the anecdotes, whether frightful, affecting, or amusing, which he heard narrated. The minute details of his recitals, the proper names with which he interspersed them, the technical descriptions of castles and country houses, of forests and caves, to which the scene was successively transported, gave to these *improvisations* so complete an air of truth, that one could scarcely retain the slightest sentiment of disbelief. On one occasion, however, Watt experienced considerable embarrassment in extricating his characters from the labyrinth in which he had somewhat imprudently involved them. One of his friends, perceiving his difficulty, from the unwonted frequency with which he applied to his snuff-box, as if to explain his pauses, and gain time for reflection, said to him, "Are you, at random, recounting a tale of your own invention?" "Your inquiry," replied the old man, "astonishes me; during the twenty years I have been so happily spending my evenings with you, I have done nothing else. Surely you did not wish to make me the rival of Robertson and Hume, when the utmost of my pretensions was to follow, at a humble distance, in the footsteps of the Princess Scheherazade, of '*The Thousand and One Nights.*'"

Every year, during a very short visit to London, and sometimes to towns not so remote from Birmingham, Mr Watt made a minute examination of every thing new which had appeared since his previous journey. From this remark, I do not except the industrious fleas, and the puppet theatricals (marionettes); for the illustrious engineer visited them with all the enthusiasm and joy of a schoolboy. In following, even now, the itinerary of these annual progresses, we should find, in many places, brilliant traces of Mr Watt's visits. At Manchester, for example, we may see the Ram *(Belier hydraulique)*, at the suggestion of our associate, helping to raise the water of condensation of a steam-engine to the feeding reservoir of the boiler.

Watt usually resided at his country-seat near Soho, called Heathfield, which he bought about the year 1790. The respectful veneration which my friend Mr James Watt cherishes for every thing that recalls the memory of his father, procured for me the satisfaction of examining, in the year 1834, the library and furniture at Heathfield, in the precise state in which the illustrious engineer left them. A second property, on the picturesque banks of the Wye in Wales, furnishes to travellers multiplied proofs of the refined taste of Mr Watt and his son, for the improvement of roads, plantations, and all kinds of agricultural operations.

The health of Mr Watt had improved with his years; and his intellectual faculties retained all their vigour to the last. At one time our associate imagined that they were declining, and, in keeping with the seal he had adopted (an eye surrounded with the word *observare*), he determined to satisfy his doubts by making observations on himself; and accordingly, when upwards of seventy years of age, he determined to select some kind of study on which he might try his powers, and for a time was in despair, because he could find no subject that was new to him. At length he thought upon the Anglo-Saxon tongue, which is a difficult language; and immediately it became the subject of the desired experiment, when the facility with which he mastered it, soon convinced him there was no ground for his apprehensions.

During the last few months of his life, Mr Watt was engaged in the construction of a machine intended to copy rapidly, and with mathematical precision, pieces of statuary and sculpture of all dimensions. This machine, of which it is to be hoped that the arts will not be deprived, must be nearly completed. Many of its productions, upon the whole very satisfactory, are now to be seen in various private collections, both of Scotland and England. The illustrious engineer presented them to amateurs somewhat facetiously, as the first attempts of a young artist entering upon the eighty-third year of his age.

Of that eighty-third year, our associate was not permitted to see the end. In the commencement of the spring of the year 1819, alarming symptoms appeared, which defied all the powers of medicine. Mr Watt was himself perfectly aware of his situation, and often remarked to the numerous friends who visited him, that he was deeply affected with the strong attachment they manifested towards him, and that he was the more anxious to thank them, because he had arrived at his last illness. His son appeared to him not sufficiently resigned; and every day he sought some new occasion to point out to him with gentleness, kindness, and tenderness, the many causes of consolation which the circumstances of the inevitable event still presented. That mournful event happened on the 25th of August 1819.

Watt was interred in the burying-ground of the parish church of Handsworth, near Birmingham, in Staffordshire. Mr James Watt, whose distinguished talents and noble sentiments, for nearly twenty-five years, enhanced the happiness of his father's life, has erected to him a splendid Gothic monument which makes the church of Handsworth extremely remarkable. In the centre of the structure, there is an admirable statue, in marble, by Chantrey, which is a very faithful representation of the noble features of the original.

A second statue, likewise in marble, from the hand of the same master, has also, by filial affection, been placed in one of the halls of the celebrated university, where, in his youth, the artist, yet unknown and exposed to the persecution of the corporations, received encouragement so flattering, and so well merited. Nor has the town of Greenock forgotten that it can boast of being the birthplace of Watt. Its inhabitants have ordered, at their own expense, a marble statue to the illustrious mechanist. It is to be placed in a beautiful library, built on a site presented gratuitously by the late Sir Michael Shaw Stewart, and in which will be united the books of the public library, and the Collection of Works of Science which Watt had presented to the establishment during his lifetime. This building has already cost L.3500, to which Mr James Watt has liberally contributed. A fine colossal statue in bronze, placed upon a beautiful pedestal of granite in George's Square, Glasgow, demonstrates to every one how proud this capital of Scottish industry is of having been the cradle of Watt's discoveries. Finally, the portals of Westminster Abbey have been opened at the voice of an imposing union of subscribers; and there stands a colossal statue of WATT in Carrara marble, the chef-d'œuvre of CHANTREY (bearing an inscription by Lord BROUGHAM),* and one of the principal ornaments of the English Pantheon. There is unquestionably much art in thus uniting the illustrious names of Watt, Chantrey, and Brougham, upon the same monument. But far be it from me to make this a ground of blame. Happy rather that people who thus avail themselves of opportunities to honour their illustrious dead.

We have thus given an account of five grand statues which have been erected in a short time to the memory of Watt. And must we now confess, that these tributes of filial love, and public gratitude, have excited the disapprobation of some narrow-minded beings, who, remaining stationary themselves, fancy they thereby arrest the advance of ages. According to them, warriors, and magistrates, and statesmen (though I venture to assert they will surely not include all of this latter class), have alone a right to statues. I do not know if Homer and Aristotle, if Descartes and Newton, would appear in the eyes of these new Aristarcuses worthy of a simple bust; but assuredly they would refuse even a modest medal to our Papins and Vaucansons, our Watts and Arkwrights, and to other mechanists, unknown, perhaps, in a certain circle, but whose renown will go on augmenting from age to age with the progress of knowledge. Since such heresies as these are openly avowed, we must not disdain to combat them. It is not without reason that public opinion has been styled a sponge for prejudices. But prejudices are like hurtful plants: the slightest effort suffices to eradicate them, if they be at once attended to; on the contrary, they grow with time, become inveterate, extend far and near, and their numerous ramifications seize upon every thing that comes within their reach.

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* We subjoin the inscription :—

Not to perpetuate a name
which must endure while the peaceful arts flourish,
but to shew
that mankind have learnt to honour those
who best deserve their gratitude,
the King,
his Ministers, and many of the Nobles
and Commoners of the Realm,
raised this Monument to
JAMES WATT,
who directing the force of an original genius,
early exercised in philosophical research,
to the improvement of
the Steam Engine,
enlarged the resources of his country,
increased the power of man,
and rose to an eminent place
among the illustrious followers of Science,
and the real benefactors of the world.
Born at Greenock MDCCXXXVI.
Died at Heathfield in Staffordshire, MDCCCXIX.

If this discussion offend the self-love of some, I must remark it has been provoked. The men of science of our day are not those who have complained that they saw not the great authors, whose inheritance they cultivate, figure in the long ranges of colossal statues, which the authorities proudly elevate on our parapets, and places of public resort; for well they know that these monuments are fragile, that hurricanes shake and overturn them, and that the very vicissitudes of the seasons suffice to destroy their contours, and to reduce them to shapeless blocks. But they too have their statuary and their paintings in the printing-press. Thanks to this admirable invention when the works of science and imagination possess real value, they may defy time and political revolutions. Neither fiscal regulations, nor commotions, nor all the terrors of despots, can hinder such productions from crossing the most carefully guarded frontiers. A thousand vessels transport them, under a variety of forms, from hemisphere to hemisphere. They are studied at one and the same time in Iceland and Van Diemen's Land; they are read in the little circle of the humblest cottage, and in the most brilliant saloons of palaces. The author, the artist, and the engineer, are recognised and appreciated throughout the world by all that is most noble and elevated in man—by judgment, mind, and intelligence. That individual would be foolish indeed, who, occupying such a commanding position, should ever wish that his lineaments, traced in marble or in bronze, even by the chisel of David, should ever be exposed to the gaze of idle loungers. Such honours as these, I repeat, a man of scientific or literary celebrity, or an artist, cannot envy, although he can never admit that he is unworthy of them. Such, at all events, is the decision suggested to my mind by the discussion I am about to submit to your attention.

It is a circumstance passing strange, that our opponents have been led to advance such haughty pretensions, precisely on the occasion of the erection of five statues, which have not withdrawn a single farthing from the public treasury. It is far, however, from being my intention to avail myself of this indiscretion. I prefer to consider the question in the abstract, namely, as already stated, the alleged superiority of arms over literature, science, and the arts. And here we must not be deceived; for, if magistrates and governors be associated with military men, it is only that they may be a passport to them.

The extreme shortness of the time I can now devote to this discussion, impresses upon me the necessity of being concise and methodical. That there may be no mistake, then, as to my opinions, I very explicitly declare that independence and national liberty are, in my estimation, the first of human blessings,—that to defend them against foreign and intestine foes is the first of duties, and that to have maintained them—at the price of one's blood—establishes the first claim to public gratitude. Raise, then, splendid monuments to the brave men who fell on the glorious ramparts of Mayence, on the immortal fields of Zurich, and of Marengo, and assuredly my offering will be readily paid; but, at the same time, do not require that I shall do violence to my reason, and to those sentiments which nature has planted in every bosom, nor expect that I shall ever agree to place all military service on the same distinguished level. What Frenchman, possessed of a spark of feeling, even of the times of Louis XIV., would willingly have sought an example of courage, either in the cruel scenes of the Dragonnades, or in the flaming whirlwind which consumed the towns and villages, and rich domains of the Palatinate? Not long since, after a thousand prodigies of patience, ability, and bravery, our valiant soldiers forced Saragossa, already more than half in ruins, and reached the portals of a church, where a priest was heard to exclaim, in the ears of the devoted crowds, these sublime words:—"Spaniards! here I celebrate your obsequies!" I do not know but that, at this moment, the true friends of our national glory, balancing the several merits of the conqueror and the conquered, would gladly have seen them exchange places?

Let morality, if you choose, be put entirely out of the question. Bring to the bar of conscientious criticism the personal claims of certain gainers of battles, and believe that, after having assigned a just share to accident (an ally this of which little is said, because it is dumb), there will be but few heroes who will appear worthy of this high-sounding name.

Were it at all necessary, I should not decline entering into an examination of details, few as have been the opportunities which my purely academical occupations have supplied for collecting accurate documents on the point. I might, for example, cite from our own annals a modern battle,—a battle gained too, the official accounts of which described all as foreseen and anticipated, planned after reflection, and executed with consummate ability, but which, in truth, was gained by the spontaneous movements of the soldiery, without orders from the general, upon whom the honours were heaped, and who neither gained the day, nor knew how it was won.

That I may escape the common reproach of incompetency, I shall summon to my help, in support of the philosophical thesis I maintain, no others than warriors themselves. We shall then see, how much they valued intellectual labours, and how enthusiastically they appreciated them; and we shall find, that in their real opinions, works of genius never occupied the second rank. Confined within narrow limits, I shall endeavour, for number and novelty, to substitute celebrity, and shall only cite the opinions of Alexander and Pompey, of Cæsar and Napoleon.

The admiration of the Macedonian conqueror for Homer is an historical fact. Aristotle, at his command, undertook the revision of the text of the Iliad. This corrected copy became his favourite and most esteemed book; and when, in the centre of Asia, amidst the spoils of Darius, a magnificent coffer, of gold, pearls, and precious stones, appeared to excite the cupidity of his lieutenants,—"Let it be reserved for me," exclaimed the conqueror of Arbela, "that I may put my Homer in it. It is the best and most faithful of my counsellors during my military operations; and it is, moreover, just, that the richest production of the arts should be used for the preservasion of the most precious production of the human mind." The sack of Thebes had previously demonstrated more clearly still, the unlimited respect and admiration Alexander entertained for literature. One single family alone, of this populous town, escaped death and slavery, and it was the family of Pindar. A solitary mansion remained erect amidst the ruins of temples, palaces, and private dwellings, and it was the house where Pindar— not that where Epaminondas—was born!

After the termination of the Mithridatic war, when Pompey went to visit the celebrated philosopher Posidonius, he prohibited the lictors from knocking at his door with their rods, according to their practice. Thus bowed, adds Pliny, before the humble dwelling of a sage, the fasces of him who had beheld the east and the west prostrate at his feet!

Cæsar, whom literature may well claim as her son, has clearly, and in many passages of his immortal Commentaries, indicated the relative places which the various kinds of faculties—so largely possessed by himself—held in his estimation. How brief and rapid his accounts of combats and battles! On the contrary, he considers no detail he can lavish upon the description of the famous bridge along which his army so unexpectedly crossed the Rhine as superfluous. And why? Because here success depended upon conception alone; and the designs were all his own. The preference which

Cæsar assigned to the several events of war, has been pointed out; what he chiefly boasted of was a moral influence. "*Cæsar harangued his army,*" is almost always the first clause of his description of a battle won. "Cæsar did not arrive in *time to address his soldiers, and exhort them to good conduct,*" is the habitual accompaniment of the recital of a surprise, or of a temporary defeat. The general invariably disappears before the orator, *and truly*, remarks the judicious Montaigne, *his tongue often did him wondrous service.*

And now, without transition, without dwelling upon the well-known exclamation of the great Frederick—"I had rather have written Voltaire's *Siècle de Louis XIV.* than gained a hundred battles;"—I come to Napoleon. I must be brief, not even alluding to the celebrated proclamation, written under the shade of the Pyramids, by a *member of the Institute,* and General-in-Chief of the Army of the East; nor to the treaties of peace, in which the monuments of art and of science were the ransom-price of the vanquished; nor to the high esteem in which the General, now become Emperor, ever held Lagrange, Laplace, Monge, and Berthollet, and the riches and honours with which he loaded them. An anecdote which is little known, will conduct us more directly to the point.

Most people know something of the decennial prizes. The four classes of the Institute had drawn up rapid analyses of the progress of science, literature, and the arts; and the Presidents and Secretaries were to be called upon to read these documents to Napoleon, before the Dignitaries of the Empire and the Council of State. The 27th of February 1808 was the day appointed for the assembling of the *Académie Francaise;* and, as may be supposed, the meeting was more numerous than usual, for who does not suppose himself a sufficient judge in matters of taste? M. Chénier was the appointed orator. He was listened to with religious silence; but on a sudden the Emperor interrupted him, and with his hand upon his heart, and his body inclined, his voice trembling with manifest emotion, he exclaimed, "This is too much gentlemen, too much; you overwhelm me; I cannot find words to express my gratitude?"

I will leave it to yourselves to conceive the deep surprise of the crowd of courtiers who were the witnesses of this scene; those very individuals who, from step to step, had gone so far in their adulation as to tell their master, and apparently without exciting his astonishment or rebuke, that "When God had created Napoleon, he felt need of repose?"

What, then, were the words which went so straight—so pointedly to the heart of Napoleon? They were the following: "In our camp, where, far from the calamities of the interior, the national glory was preserved unsullied, sprung up another kind of eloquence till then unknown in modern times. Concerning it there can be no dispute. When, in ancient authors, we read the harangues of the most renowned captains, we are usually called to admire only the genius of the historian. But here the heart-stirring addresses still exist, and may be collected without trouble. These beautiful proclamations emanated from the army of Italy; where the conqueror of Lodi and Arcole, whilst creating a new art of war, gave existence also to military eloquence, of which he will ever remain the model." On the 28th of February, the day subsequent to the celebrated meeting, the particulars of which I have now traced, the *Moniteur*, with its *accustomed fidelity*, published the Emperor's answer to the discourse of Chénier. It was cold, formal, and insignificant, and had, in short, all the characters, others might say, all the qualities of an official document. It made no allusion to the incident I have related; —miserable concession to predominating opinion, and the susceptibilities of the Staff.

The master of the world, to avail myself of Pliny's expression, yielding for a moment to his real feelings, lowered his fasces before the literary compliment paid to him by the Academy.

These reflections, upon the comparative merit of the philosopher or author, and the warrior, although they have been suggested to me chiefly by what is said, and what passes, around us, will not be wholly inapplicable to the native country of Watt. I lately travelled extensively throughout both England and Scotland. The kindness I received authorized me to ask questions, so searching, distinct, and direct, that, in other circumstances, they would have been excusable only in the

President of a board of inquiry. Being even then deeply preoccupied with the obligation under which I lay, to deliver, on my return, a judgment concerning the celebrated mechanist; and already somewhat anxious at the thought of the solemnity of that assembly to which I now address myself, I had prepared this inquiry, "What is your opinion of the influence which Watt exercised upon the wealth, the power, and the prosperity of England?" I do not exaggerate when I say that I have addressed this question to more than a hundred people, belonging to all classes of society, and to all shades of political opinion, from the most restless radical to the most determined conservative; and the response has always been the same. Each placed the services of our associate above all comparison; and almost every one quoted the speeches, made at the meeting which agreed to the erection of the statue in Westminster Abbey, as the faithful and unanimous expression of the sentiments of the British nation. What, then, was the tone of these speeches?

Lord Liverpool, the first minister of the crown, designated Watt as one of the most extraordinary men to whom England had given birth, and one of the greatest benefactors of the human race. He declared that his improvements had increased to an incalculable degree the resources of his country, and even those of the whole world. Then looking at the question in a political point of view, "I have lived," said he, "in times, when the success of a campaign, or even of a war, has depended upon the possibility of dispatching our squadrons from port, and when contrary winds prevailed for whole months, and completely disappointed the anxious wishes of Government. Thanks to the steam-engine, such vexatious difficulties are now for ever at an end."

"Again," exclaimed Sir Humphry Davy, "cast your eye upon the metropolis of this mighty empire, upon our towns and villages, our arsenals and manufactories,—examine the subterranean excavations and the works that are executed on the surface of the earth,—contemplate our rivers, our canals, and the ocean which surrounds our shore, every where will you find tokens of the enduring beneficial labours of this great man." "The genius," added the illustrious President of the

Royal Society, "which Watt has displayed in his admirable inventions, has contributed more to demonstrate the practical utility of science, to aggrandize the power of man over the material world, and to multiply and spread wide the conveniencies of life, than the labours of any other individual in modern times." Davy, in fact, did not hesitate to place Watt in a more elevated position even than Archimedes.

Mr Huskisson likewise, the President of the Board of Trade, proclaimed, that, regarded in reference to the prosperity of the human race, the inventions of Watt appeared to him to merit *the highest possible admiration.* He explained in what way the economy of labour, the indefinite multiplication and the extreme cheapness of the productions of industry, contributed to advance knowledge, and promote its wide extension. "The steam-engine," he remarked, "in the hands of man is not only the most powerful instrument he can employ in changing the appearance of the physical world, it also acts as a moral and irresistible lever in pushing forward the grand cause of civilization." In this point of view Watt appeared to him distinguished among the chief benefactors of mankind. And, as an Englishman, he did not hesitate to say, that without the inventions of Watt, the British nation could never have sustained the immense expense of its last war with France.

The same idea occurs in the speech of another member of Parliament, who expresses his sentiments in no less decided terms: I allude to Sir James Mackintosh. "It is the disveries of Watt which have enabled England to sustain the most arduous and dangerous conflict in which she has ever been engaged. All things considered, I declare, without hesitation, that no one had ever more urgent claims than Watt to the homage of his country, or to the veneration and respect of future ages."

We shall now turn to the result of numbers and figures, which, as it appears to me, are still more eloquent than the various passages we have just been perusing. The younger Mr Boulton informs us, that, in the year 1819, the establishment at Soho alone, had manufactured of Watt's machines, a number whose steady labour would have required not fewer than one hundred thousand horses; and that the saving resulting from the substitution of these machines for animal labour, amounted annually to more than L.3,000,000 Sterling. Throughout England and Scotland at the same date, the number of these machines exceeded ten thousand. They effected the work of 500,000 horses, or of three or four millions of men, with an annual saving of from L.12,000,000 to L.16,000,000 Sterling. These results must by this time be more than doubled.

See here, then, in a scanty abridgment, what was said of Watt by statesmen, philosophers, and manufacturers, the most capable of appreciating his merits. Gentlemen, this creator of six or eight millions of labourers,—of assiduous and indefatigable labourers, among whom authority is never required to repress either coalition or commotion, and who labour for a halfpenny a-day;—this man, who, by his brilliant discoveries, afforded England the means of supporting a most furious struggle, during which her very nationality was at stake;—this second Archimedes, the benefactor of his race, whose memory future generations will for ever bless,—what, I inquire, was done for him during his life-time?

The peerage is in England the first of dignities, the highest of rewards; and you will naturally suppose that Watt was created a Peer. So far was this from being the case, that it was never even thought of! Were we to speak the truth, we should say, so much the worse for the Peerage. Such a neglect, however, in a nation so justly proud of its illustrious men, could not but greatly astonish me. When I inquired into the cause of this neglect, what think you was the response? Those dignities of which you speak, I was told, are reserved for naval and military officers, for influential members of the House of Commons, and for members of the aristocracy. "*It is not the custom,*" it was said, and I quote the very phrase, "to grant these honours to scientific and literary men, to artists or engineers!" I well knew it was not the custom in the reign of Queen Anne, because Newton was never a Peer of England. But after a century and a half of progress in science and philosophy, when all of us, within the short span of life, have seen monarchs banished, forsaken, proscribed, and replaced upon their thrones by mere soldiers of fortune, who have hewn out their renown by their swords, surely I might be permitted to hope that the time had passed when it would be attempted to divide men into exclusive classes; that, at all events, it would not be declared openly, and in the style of the inflexible code of the Pharaohs: Whatever may be your services, your virtues, or your acquirements, not one of you shall ever rise above the level of your caste; in a word, that such a senseless custom (since custom it is) should no longer be permitted to disfigure the institutions of a great people.

Let us hope better things of the future. The time will come when the science of destruction shall decline before the arts of peace; when the genius which multiplies our powers, which creates new products, and dispenses comfort throughout immense masses of our population, shall occupy, in general esteem, the place which reason and sound sense have even now assigned to it.

Watt will then appear before the grand jury of the population of the two hemispheres. They will see him, assisted by his steam-engine, penetrating in a few weeks into the bowels of the earth, to depths, which, before his time, could only have been reached after an age of the most difficult labour; he will there clear out spacious galleries, and free them, in a few minutes, from the vast volumes of water which daily overflow them, and thus will he procure from the virgin earth those inexhaustible mineral riches which nature has there deposited. Uniting delicacy to power, Watt will be seen twisting with the same success, the huge folds of the colossal cable by means of which the stately vessel rides secure amid raging seas, and the microscopic filaments of those laces and airy gauzes, upon which fashion ever so much depends in the preparation of her light but fascinating adornments. A few strokes of the same machine will drain vast marshes, and give them up to husbandry; and districts already fertile will by it be freed from the periodic influence of those deadly miasmata produced by the scorching heat of the summer sun. Those great mechanical powers, which are only to be found in mountainous regions, at the foot of rapid cascades, will now, thanks to the ingenuity of Watt, be reared at will, without difficulty and without incumbrance, in the centre of towns, and in every story of a building. The intensity of these powers will be regulated by the mechanic's will, and will not depend, as heretofore, upon the most unsteady of natural causes,—atmospheric influence. The different branches of each manufacture may be united in a common enclosure, and even under the same roof. The productions of industry, whilst they are thus improved in quality, will be diminished in price. Population, well fed, well clad, and comfortably lodged, will increase with rapidity,—it will cover with elegant dwellings every region, even those districts which have been justly styled the Steppes of Europe, and which the barrenness of ages seems for ever to have condemned to remain the exclusive domain of the *feræ naturæ.* In a few years insignificant hamlets will become important cities; and, in a short while, such towns as Birmingham, where, a few years since, one could scarcely count thirty streets, will take their place among the largest, most beautiful, and richest towns of a powerful kingdom.

Transferred to our ships, the steam-engine will replace an hundredfold, the efforts of the triple and quadruple banks of rowers, from whom our fathers required an extent and kind of labour, ranked among the punishments of the greatest criminals. With the help of a few bushels of coals Man will overcome the elements, and will make light of calms, contrary winds, and even storms. Transport will become much more rapid,— the time of the arrival of the steam-vessel will be as regular as that of our public coaches; and we shall no longer have occasion to remain on the coast for weeks, or even months, the heart a prey to cruel anxiety, watching, with anxious eye on the distant horizon, for the uncertain traces of the vessel which is to restore to us a father or a mother, a brother or a friend. In fine, the steam-engine, conveying in its train thousands of travellers, will run, upon railroads, more swiftly than the best race-horse, loaded only with its diminutive jockey.

This, gentlemen, is a very abridged sketch of the benefits bequeathed to the world by the machine of which Papin supplied the germ in his writings, and which, after so many ingenious exertions, Watt carried to such admirable perfection. Posterity will assuredly not degrade them to the level of other labours which have been too much commended, and whose real influence, weighed by the tribunal of reason, will for ever remain circumscribed within the confined circle of a few individuals and a limited space of time.

We have long been in the habit of talking of the age of Augustus, and of the age of Louis XIV. Eminent individuals amongst us have likewise held that we might with propriety speak of the age of Voltaire, Rousseau, and Montesquieu. I do not hesitate to declare my conviction, that, when the immense services already rendered by the steam-engine shall be added to all the marvels it holds out to promise, a grateful population will then familiarly talk of the ages of Papin and of Watt!

A biography of Watt, intended to form a part of our collection of memoirs, would certainly be incomplete, did it not contain an enumeration of the academic titles with which the illustrious engineer was invested. The list will, moreover, occupy but a few lines. Watt became a member of the Royal Society of Edinburgh in the year 1783; of the Royal Society of London in 1785; of the Batavian Society in 1787; a correspondent of the Institute in the year 1808; and in 1814, *L'Academie des Sciences* of the Institute, conferred upon Watt the highest honour it can bestow, by naming him *one of its eight* foreign associates. By a spontaneous and unanimous vote the Senate of the University of Glasgow conferred on Watt, in the year 1806, the honorary degree of LL.D.

On Machinery considered in Relation to the Prosperity of the Working Classes.[*] By M. Arago.

Many individuals, without questioning the genius of Watt, regard the improvements on account of which the world is his debtor, and the great impulse they have given to the labours of industry, as a social calamity. Were we to believe them, the adoption of every new machine inevitably increases the inconveniences, and adds to the miseries of our artisans. All those wonderful mechanical combinations which we are in the custom of admiring for the regularity and harmony of their movements, and for the energy and delicacy of their effects, are, in their opinion, only instruments of evil, which the legislator ought to proscribe with a just and implacable severity.

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[*] In writing the following chapter, I thought that I might avail myself without scruple of the numerous documents I have collected, whether in my occasional intercourse with my illustrious friend Lord Brougham, or

Conscientious opinions, and especially when associated with feelings of philanthropy, should ever have a claim to attentive examination. Nay, I will add, that from me such an examination is an imperative duty. I should, in fact, neglect that aspect of the labours of our illustrious associate which is most worthy of public admiration, were I not, far from subscribing to the criticisms of prejudice, to hold up such labours to the attention of men of property, as the means the most powerful, the most direct, and the most efficacious for relieving the operatives of their hardest sufferings, and for making them participate in all the blessings which appeared to be the peculiar inheritance of the rich.

When we have to make a choice between two propositions which are diametrically opposed to each other—when the one being true, the other must necessarily be false, and when nothing, at the first glance, seems to indicate a rational choice between them, geometricians are in the habit of taking up

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in those works which his Lordship has published, or which have appeared under his patronage. Were I to believe the criticisms which various persons have published since the reading of this eloge, I have, in endeavouring to combat the opinion that machinery is injurious to the working classes, been attacking a worn out prejudice which has no longer any real existence. Could I believe this to be the case, I would willingly suppress all my reasonings, good or bad. But unfortunately, the letters which worthy workmen frequently address to me, whether as Academician or Deputy, and still more the dissertations *ex professo*, and quite recent, of several political economists, leave me no doubt as to the necessity of affirming now, and of repeating upon every becoming occasion, that machinery has never been the real cause of the sufferings of one of the most numerous and interesting classes of society; that its destruction would only aggravate suffering; and that it is not in this quarter we shall find the remedy for evils, which I regret from the bottom of my heart.

these contrary propositions, of following them out minutely through their several ramifications, and so arriving at their ultimate logical results; and the proposition which is incorrect, and it alone, seldom fails by this process to lead to consequences which a correctly constituted mind cannot admit. Let us employ for a moment this method of examination, of which Euclid so often availed himself, and which is so justly termed the *reductio ad absurdum.*

The opponents of machinery would annihilate it, or at least would greatly restrict its employment, to preserve, they say, more work for the labouring classes. Let us for a moment adopt this view, and we shall find that the anathema extends far beyond machines properly so called.

And we must begin by taxing our ancestors with the greatest improvidence. If, instead of founding the city of Paris, and continuing to extend it on both banks of the Seine, they had built it upon the plain of Villejuif, then, for ages, the corporation of water-carriers would of all others have been the best employed, the most necessary, and the most numerous. The political economists, therefore, with whom we are now contending, should consult the interest of these water-carriers. To divert the Seine from its course is by no means an impossibility; they should, therefore, propose the accomplishment of this great work—they should open a subscription to divert the river from Paris; and the general laugh would then teach them that the method of the *reductio ad absurdum* is not without its use even in political economy; and the workmen themselves, in their right senses, would tell them that it is the river which has created that immense Capital where they find so many sources of occupation, and that without it, Paris would probably still have been only another Villejuif.

Up to the present time, the Parisians have always been congratulated upon their proximity to those inexhaustible quarries, whence, for many generations, have been procured the materials employed in the construction of their temples, their palaces, and their private dwellings. But all this is mere illusion! The new political economy will prove to us, that it would have been eminently advantageous had all our stone and lime been found no nearer than Bourges, a distance of 120 miles. In this case, count upon your fingers, if you can, the number of workmen, whose employment would have been necessary, to convey to the stone-yards of the capital, the stones which the builders have required for five centuries, and you will obtain a prodigious result; and however little satisfied you may be with novel ideas, yet you may rejoice to your heart's content upon the delight which such a state of things would create among the day labourers!

The capital of a powerful kingdom, not very distant from France, is traversed by a majestic river, which even ships of war ascend in full sail. Innumerable canals in all directions intersect the surrounding country, and transport, at little expense, packages of the largest bulk. A complete net-work of excellent roads, most admirably kept, leads to the most distant parts of the country. In addition to these great gifts of nature and of art, the capital enjoys, what every one must now call *an advantage* of which Paris is deprived, for the stone-quarries essential to building are not in its vicinity, but are found only at a distance. Here, then, is the Utopia of the new economists realized. They may now calculate the hundreds of thousands, nay, the millions of quarrymen, boatmen, carters, and stone-cutters, who must unceasingly be occupied in raising, transporting, and preparing all the variety of stones which are required in the construction of the immense number of buildings, which are every year added to this great metropolis. But stop! they may spare their pains, for it happens that in this city—as it would happen in Paris, deprived of its rich quarries—that stone being very expensive, is not used, and brick is everywhere substituted in its place.

Thousands of workmen every day execute at the surface and in the bowels of the earth prodigious labours, which it would be necessary totally to abandon, if certain machines were relinquished. One or two examples will suffice to make this truth sufficiently apparent. The daily removal of the water which rises in the galleries of the Cornish mines requires a power of fifty thousand horses, or of three hundred thousand men. I ask if the wages of three hundred thousand workmen would not absorb all the profits which the mining operations might produce? But the question of wages and profits may touch a tender point; and therefore I turn to other considerations, which, however, lead to the same conclusion. A single copper-mine in Cornwall, one of those known as the *Consolidated mines*, requires a steam-engine of the power of more than three hundred horses constantly at work, and thus every twenty-four hours realizes the labour of one thousand horses. Concerning this, the assertion cannot be doubted, that no means could possibly be found beneficially and simultaneously to apply the strength of more than three hundred horses, or two or three thousand men, around the mouth of the shaft of the mine. To proscribe, therefore, the action of the steam-engine of the *Consolidated mines*, would be to reduce to a state of inactivity a great number of workmen whose labours are now rendered available; it would be to declare that the copper and tin mines of Cornwall must for ever remain buried under a mass of soil, rock, and liquid, many hundred yards deep. The proposition, brought to this form, could certainly have few defenders; but the form is nothing, whilst the substance remains the same.

If, from operations which require the greatest development of power, we turn to the examination of different products of industry whose delicacy of parts and regularity of form have ranked them among the wonders of art, the insufficiency, and even the inferiority of our organs, compared with the ingenious combinations of machinery, are equally striking to all. Where, for example, is the skilful spinner who, from a single pound of raw cotton, could produce a thread one hundred and fifty miles long, as can the mule-jenny?

I am not ignorant of what certain moralists have said about the uselessness of muslins, laces, and tulles, which these slender threads are employed to manufacture; but I need only remark, that the most perfect *mule-jennies* require the continued superintendence of a great number of workmen; that the only object with them, is to manufacture productions which will sell; and that, finally, if luxury be an evil, a vice, or even a crime, it should be ascribed to the buyers, and not to the poor workers, whose means of existence, I believe, would be hazarded, if they employed their strength in making for the ladies, coarse stuffs, instead of fashionable tulles.

Let us now, however, leave these details, and come to the essential merits of the question. "We must not," said Marcus Aurelius, "receive the opinions of our fathers, as do mere infants, for the simple reason that our parents held them." This assuredly just maxim should not at the same time prevent us from thinking, or, at all events, from presuming, that those opinions, concerning which no objection has been raised since the origin of society, are conformable both to reason and general interest. Well, then, upon the opinion so much debated concerning the utility of machinery, what were the unanimous sentiments of antiquity? Its ingenious mythology will inform us. The founders of empires, the first of legislators, the conquerors of those tyrants who oppressed their country, received the qualified title of demi-gods; and it was among these same gods that were placed the inventors of the spade, the sickle, and the plough.

But instantly, I hear some of our adversaries declaiming about the extreme simplicity of the instruments I have cited,—refusing them the name of machines,—designating them mere tools, and obstinately entrenching themselves behind this distinction. To this we might reply, that such a distinction is puerile; and that it is impossible to say with precision where the tool ends, and the machine begins. It is of more importance, however, to remark, that the murmurers against machines have never said one word of their greater or less degree of complication. If they reject them, it is because, by their means, one workman performs the labour of many; and will they venture to maintain that a knife, a gimlet, a file, or a saw, does not give a marvellous facility of action to the man who uses them; and that his hand, so supplied, can do the work of many hands armed only with their nails.

Those labourers were not arrested by this sophistical distinction between tools and machinery, who, seduced by the execrable theories of some of their pretended friends, ran through certain counties of England, in the year 1830, vociferating *Death to machinery!* Rigorous logicians, they broke the sickle in the farm-yard, destined for reaping, the flail employed in thrashing, and the sieve with which the grain was cleaned. Who can deny, that the sickle, the flail, and the sieve are, in truth, means for abridging labour? Not even the spade, pick-axe, plough, and driller, found favour with this blind herd; and, if one thing in the history of this mania astonished me more than another, it was, that they spared the horses, which, in reality, are a kind of machines, kept up comparatively at a cheap rate, each of which daily executes the work of six or seven labourers.

Political economy has now happily taken its place among the sciences of observation. The experiment of the substitution of machinery for manual labour has now so frequently been resorted to, that we can draw general results, though not, perhaps, quite free from accidental irregularities. These results are as follows:—

By saving the labour of man, machinery effects a reduction in price. The effect of this reduction is increased demand; and to such an extent, so great being our desire to improve our condition, that, in spite of the almost inconceivable reduction of price, the pecuniary value of the total merchandize produced, every year surpasses what it was previous to the introduction of these improvements, and the number of workmen which these employments require, *increases* with the introduction of the means of more rapid fabrication. This last result is precisely the opposite of that which the adversaries of machinery predict. At first sight, the fact appears paradoxical; but, notwithstanding, it is demonstrated beyond dispute, by an examination of the most satisfactorily determined results.

When, three centuries and a half ago, the printing-press was invented, copyists supplied books to the very small number of the wealthy who could afford this very expensive gratification. A single individual of these copyists, by means of the new invention, being able to accomplish the work of two hundred, they did not fail at the time to characterize it as an *infernal* invention, which, in a certain class of society, would reduce to beggary nine hundred and ninety-five persons out of every thousand. Let us compare the actual results, with this sinister prediction.

Manuscript books were little in demand; printed books, on the contrary, on account of their very low price, were sought after with the greatest avidity. The great works of the principal Greek and Roman authors required to be reproduced unceasingly. New ideas and new opinions gave rise to a multitude of works, some of an enduring interest, whilst others were called into existence by passing events; so that, it has been calculated that, in London, previous to the invention of printing, the trade in books afforded occupation to about two hundred individuals, whereas, now, there are, *engaged in it, tens of thousands.* And what would be the result, if, laying aside the restricted and, so to speak, the *material* view of the subject, we regard printing in its moral and intellectual aspects; if we examined its influence upon public manners, upon the diffusion of knowledge, and the progress of the human mind; if we could count the number of volumes for which we are indebted to it, which the copyists would certainly have despised, but from which genius daily draws forth the elements of its best and most pregnant conceptions? However, I must not forget that the question before us, regards the number of workmen employed in the different branches of industry.

The manufacture of cotton presents us with results still more remarkable than those of printing. At the time when an ingenious barber at Preston, by name Arkwright, who left a fortune of about one hundred thousand pounds a-year to his children, rendered the substitution of turning rollers for the spinners' fingers, useful and profitable, the annual product of the cotton manufacture of England did not amount to more than L.2,000,000, now it exceeds L.37,000,000. In the county of Lancaster alone, there is delivered, every year, to the manufacturers, a quantity of cotton thread, which twenty-one millions of expert spinners could not prepare by means of the spindle and distaff. Now, although, in the art of preparing this thread, mechanical means have been carried to their extreme limits, a million and a half of workmen find daily employment, where, previous to the inventions of Arkwright and Watt, there were not fifty thousand employed.*

A certain philosopher has exclaimed, Nothing new is now published, unless that which has been long forgotten be called such. If he intended his remark to apply only to old errors and prejudices, there would be some point in it; for all ages have been so fertile in this way, that no one can now have the advantage of priority. For instance, the pretended philanthropists of our day have not even the merit (if merit there be) of having invented the system I am now examining. For behold poor William Lea exhibiting the first stocking-frame in the presence of King James I. The mechanism appeared admirable; why then reject it? Simply from the pretext, that the working classes would suffer. Nor was France a whit wiser than England. Lea found no encouragement there, and he died in an hospital, like so many other men of genius who have had the misfortune to advance beyond their age.

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* Mr Edward Baines, the author of an esteemed work upon the history of the cotton manufacture in Britain, has had the singular curiosity to calculate the length of the thread which is annually employed in the fabrication of cotton goods, and has found that the total length equals fifty-one times the distance of the sun from the earth (fifty-one times thirty-nine millions of leagues), or about two thousand millions of leagues.

It should here be observed, that a person would very much deceive himself if he supposed that the stocking-knitters, of whom William Lea thus became the victim, were a numerous body. In the year 1583, none but individuals of high rank and large fortune wore stockings. The middle classes, instead of this part of our clothing, wore tight bandages of various stuffs. The rest of the population, that is to say, nine hundred and ninety-nine out of every thousand, went bare-legged. Now, on the contrary, out of a thousand individuals, there is not perhaps more than one who is not able, owing to their extreme cheapness, to provide a pair of stockings. And hence, an immense number of workmen in all countries, are engaged in this kind of manufacture.

Were it at all necessary, I might add, that, at Stockport, the substitution of steam for the labour of the hands in the manufacture of lace, has increased the number of workmen engaged in this branch of manufacture, and this to the extent of one-third, in a very few years.

We must now, finally, drive our adversaries from their last retreat, for they must not be allowed to say, that we have adduced our proofs solely from antiquated subjects of human industry. We accordingly remark, that their lugubrious anticipations respecting the recent engraving on steel, were not a whit less wide of the mark. A copper-plate, they observed, cannot produce more than two thousand impressions. A steel plate, which can supply a hundred thousand, without wearing, will supplant fifty copper-plates. The plainest arithmetic, therefore, they contended, demonstrates that the majority of engravers (forty-nine out of the fifty), will be forced to leave their benches,—to throw away their graver for the trowel and the pick-axe, or to implore charity on the public ways. For the twentieth time we beseech you, anticipators of evil, to remember the principal element of the problem ye pretend to solve! —consider the insatiable desire of wellbeing which nature has implanted in the heart of man;—think that the gratification of one want whets the appetite for the satisfaction of another, —that our desires increase with the cheapness of the supply which supports them, and to such an extent as to defy the creative energies of the most powerful machines.

But to return to engravings. The immense majority of the public never thought of them when they were dear, and when cheap, they are in universal demand. They have already become the ornaments of our best books, and they confer on second-rate works the best prospect of sale. Even in our almanacs, the antique and hideous representations of Nostradamus and Mathieu Laensberg have given place to picturesque views, which in a few seconds transport our immoveable citizens from the banks of the Ganges to those of the Amazon, —from the Himalayas to the Cordilleras,—from Pekin to New York. Behold too, the poor engravers whose ruin was so piteously foretold. They were never so numerous—never so well employed.

All these are unanswerable facts; and from them one consequence most assuredly does *not* follow; viz. That in the world we live in, among its inmates, such at least as nature has created them, the employment of machinery diminishes the number of workmen required in the several branches of industry. With other habits, manners, and passions, a different conclusion *might* be reached; but the probabilities on this supposition may be left to be wrought out by those who speculate upon the domestic economy of the inhabitants of the Moon, Jupiter, or Saturn.

Confined within much narrower limits, I inquire,—If, after having thus sapped the very foundation of the system of the opponents of machinery, it be at all necessary to glance at some minor points, which have been mixed up with the consideration of the subject? The Poor Law of England is the most important of these: but, from this bleeding wound, the nation has suffered since the days of Queen Elizabeth; and surely it is absurd to ascribe to the abuse of machinery, an evil which took its origin, and luxuriated for ages, before the labours of Arkwright and Watt were ever heard of. But you must concede, it will be answered, that the steam-engine, and the mule-jenny, and the carding-machine, and the printing-press, &c., these objects of general predilection, have at all events aggravated and extended the evils of pauperism? Far from it. For what are the facts? First of all, machinery has never been represented as a universal panacea. It has never been alleged that it possessed the unheard of power of dispelling error and passion from political assemblies; that it could direct the counsellors of princes in the paths of moderation, wisdom, and humanity; that it could have prevented Pitt from unceasingly intermeddling with the affairs of other countries—from every year resuscitating, in all quarters of Europe, the enemies of France,—from subsidising them with immense sums, and thus overwhelming England with a debt of hundreds of millions. This is the cause why the poor-law tax has so rapidly and so prodigiously increased. Machinery has not, and could not produce the evil. On the other hand, it has done much to moderate it,—an assertion which may be proved in few words. Lancashire is the most manufacturing county of England. In it are situated the towns of Manchester, Preston, Bolton, Warrington, and Liverpool. Here, we say, machinery has been most rapidly and most generally introduced; and with what effect? If we compare the total annual amount of the poor-law tax in Lancashire, with the amount of that raised throughout the country, and ascertain the share of each individual, we find that in this county it amounts to only one-third of the mean paid in the other counties? These cyphers, then, give no quarter to the allegations of system-makers.

Nor should the high-sounding words so often used by certain declaimers about the poor-laws, induce us to suppose that the labouring classes among our neighbours are wholly deprived of forethought and resources. A publication of very recent date has shewn that, in England alone (Scotland and Ireland being omitted), the capital belonging to operatives in the Savings-Banks now amounts to about sixteen millions Sterling! And the results observed in the principal towns are not less instructive.

There is one principle which has remained uncontroverted, amidst the animated controversies to which the science of political economy has given birth, viz. that population increases with general prosperity, and rapidly diminishes in times of wretchedness.* Now, let us place facts in juxtaposition with the principle. Whilst the mean population, during the last thirty years, has augmented in the ratio of 50 per cent., Nottingham and Birmingham, two of the most manufacturing towns, exhibit increments of 75 and 90 per cent. Finally, Manchester and Glasgow, which occupy the first rank in the British Empire for the number, size, and importance of the machines they employ, in the same period of thirty years, have seen their population augment 150 and 160 per cent.; an amount three or four times greater than in the agricultural counties, and in those towns which are not manufacturing. These figures speak for themselves, and there is no sophistry, or false philanthropy or eloquence, that can resist them.

One chief objection brought against machinery I cannot disregard. At the moment that new improvements are introduced, and when by their means manual operations are very extensively superseded, certain classes of workmen suffer from the change;—their honourable and laborious industry is annihilated at a blow; and those who, according to the previous method, were the most expert and deserving, being deficient of the qualifications which the new plan requires, are thrown out of employment, and can but rarely find other means of subsistence. These reflections are just, and I may add, that the melancholy consequences to which they refer, must frequently occur, for the mere caprices of fashion often in this way produce extensive wretchedness. If, then, I do not hence conclude that the world remain stationary, but still desire advancement in the general progress of society, I am far from maintaining that we should be indifferent to the individual sufferings, of which this progress is the momentary cause. The authorities, always watching for new inventions, seldom fail to reach them with their fiscal regulations. Would it be too much to ask, that the first contributions levied on the successful exercise of genius, should be appropriated to the opening of spacious workshops, in which the workmen deprived of employment might for a time find occupation suitable to their powers and intelligence? This course has sometimes been followed with success, and why should it not be generalized? Humanity demands it as a duty; sound policy dictates it; and, were other inducements necessary, sad events, whose history is not yet forgotten, strongly recommend it on economical grounds.

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* To this general rule Ireland is an exception; the cause of this, however, is well known.

To the objections of the theorists, who apprehended that the progress of machinery would reduce the working classes to a state of total inactivity, have succeeded difficulties of quite a different character, on which it seems indispensable we should for a few moments dwell.

By superseding, in our manufactories, every exertion of great masculine strength, machinery permits the introduction of a great number of children of both sexes; and the cupidity of their parents has a tendency to abuse the opportunity. The hours of labour are made to surpass all bounds; and, for the daily bait of a few pence, minds, which education would enlighten, are sacrificed to enduring brutalization, whilst the bodily frame is blighted for want of that development which the enjoyment of the air and sun rarely fails to bring along with it. Under these circumstances, to insist that the Legislature should put a stop to this shameful oppression—to urge forward measures calculated to contend with the demoralization which is the usual consequence of numerous meetings of the young persons employed—to endeavour to introduce, and multiply in our cottages, various implements, by which, according to the season, the labours of agriculture might be associated with those of the artisan, this would be patriotism and humanity, this would be to understand and supply the real wants of the working classes. But, on the other hand, to persist that the vast labour, which machinery can effect in a moment and cheaply, shall be performed, at a great price, by the toil of man's hand and the sweat of his brow—to assimilate the workman to the brute—daily to require from him exertions which ruin his health, and which science can obtain to the extent of a hundredfold, by means of wind, and water, and steam, this would be to recede from the grand object we have in view. It would be to abandon the poor to nakedness; to reserve a thousand enjoyments exclusively for the rich which are now common to all. It would be, in short, to return again to the ages of ignorance, barbarity, and wretchedness.

Additional Notes to M. Arago's Memoir of James Watt.[*]

"*James Watt, the father of the celebrated engineer,*" (p. 223.) We find in the minutes of the Town-Council of Greenock, under date of 3d June 1774, that Mr Watt then gave in his resignation of the office of a manager and councillor, upon which, the meeting of the magistrates and council returned him thanks for the many good services he had done to the community.

"*He combined three kinds of occupation,*" (p. 223.) He was also agent to the late Lord Cathcart in the management of his property at Greenock, who, upon Mr Watt's death, bore honourable testimony to his memory in a letter to his son.

"*Was then trying to solve a problem of geometry,*" (p. 224.) Upon this occasion Mr Watt's friend put various questions to the boy, and was astonished and gratified with the mixture of intelligence, quickness, and simplicity displayed in his answers.

"*A small electrical machine,*" (p. 224.) This must have been about the years 1750–53. It will be recollected that the Leyden Phial was invented in the years 1745–46. See Priestley's Hist. of Electricity, p. 80, ed. 1769.

"*The Machine of Marly,*" (p. 227.) This machine was erected at the village of that name on the Seine, to raise water for the town and water-works of Versailles. This was effected by means of fifteen large water-wheels, and a series of pumps, pipes, cranks, and rods, remarkable for their complexity and the noise they made in working, and which well exemplified the proverb, "*beaucoup de bruit, peu de besogne.*" In 1786–87, Mr Watt and Mr Boulton proceeded to Paris at the instance of the French Government, to suggest improvements on this machine, which were not carried into effect in consequence of financial difficulties, and the dismissal of the ministry. Since then, a steam-engine has been erected by the French to do part of the work; and two of the wheels, with their apparatus, are all that remain of this cumbersome machinery.

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[*] We are indebted to Mr Muirhead, Advocate, Edinburgh, a relative of Mr Watt, for the "Additional Notes."—Editor.

"*Adam Smith, Black,** *and Robert Simson,*" (p. 228.) Dr Dick, the professor of Natural Philosophy, might also, with propriety, have been included in this number. Mr Watt and Mr Robison were always accustomed to speak of him as a most able man. He was also Mr Watt's strenuous friend; and it was through his recommendation that he went to Mr Morgan.

"*He made out the scale of temperament,*" (p. 230.) See the article *Temperament* in the Encyclopædia Britannica, which is given in Brewster's edition of Robison's Mechanical Philosophy, vol. iv. p. 412.

"*Another and a still more important change introduced by Captain Savery, will more appropriately find a place in the remarks we shall presently devote to the labours of Papin and Newcomen.*"—(p. 242.)

We do not find, however, that farther mention is made under the heads referred to, of any other improvements by Savery. But the fact is, that Savery's engine consisted of *two* distinct *principles*; raising water by the *condensation of steam*, and likewise by the *expansive power of steam*. The steam from the *detached* boiler was let into a vessel called a receiver, and having driven out the air, was condensed by the affusion of cold water, and a partial vacuum formed. A communication being then opened with a suction-pipe, 24 feet in height, the lower end of which was placed in a cistern or reservoir of water, that water was forced upwards, by the pressure of the atmosphere, into the receiver. When nearly filled, the communication with the suction-pipe was shut off, and the steam readmitted into the receiver, and by its expansive power, forced the water contained in it up an ascending, or as he called it, a force pipe. This second operation is similar to that indicated by Solomon de Caus and by the Marquis of Worcester, but which, as far as is certainly known, Savery was the first to practise. The prior operation, that of raising the water into a vacuum formed by the condensation of steam, we believe to have been original with Savery. For although Papin described in the *Acta Eruditorum* of Leipzig for 1690, the condensation of steam in a cylinder having a piston, it was a different application of the principle; and it is not likely that Savery knew of it when he invented his engine in 1696, or perhaps sooner. Indeed, Papin had the candour to acknowledge, says Robison, that the invention was made without communication with him. When we consider the whole of the contrivances invented by Savery, including the separate boiler, we cannot but accord him the praise of very great ingenuity, independent of the merit of having made THE FIRST WORKING STEAM-ENGINE.

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* An interesting letter by Watt, containing an account of his connection with Dr Black and Professor Robison, as well as remarks on the origin of his improvements upon the Steam-Engine, will be found in the Edinburgh Philosophical Journal, vol. ii. p. 1.—Editor.

"*The Miner's Friend.*" (p. 243.) Mr Robert Stuart, p. 34 of his History of the Steam-Engine, 1824, says, quoting from Robison, "Savery obtained his patent in 1698, after a hearing of objections, * * but, besides this, he had erected several of his engines *before* he obtained his patent, and," continues Mr Stuart, "published an account of his engine in 1696, under the title of "*The Miner's Friend,*" and "*a Dialogue,*" by way of answer to the objections which had been made against it; both were printed in one volume in 1702." We have not seen the publication of 1696; but we observe that in that of 1702, he says he worked a small model before some members of the Royal Society 14th June 1699.

"*This problem,*" &c., (p. 252.) See Mr Watt's own interesting account in Brewster's edit. of Robison's Mechanical Tracts, vol. ii., *Steam-Engine*, pp. 113 to 120.

"*He enclosed the metallic cylinder,*" &c., (p. 254.) Mr Watt's words, in his specification of 1769, are, "first, by enclosing it (*i. e.* the cylinder) in a case of wood, or any other materials that transmit heat slowly; secondly, by surrounding it with steam, or other heated bodies; and, thirdly, by suffering neither water, or any other substance colder than the steam, to enter or touch it during that time." In point of practice, he surrounded the cylinder with a metallic case containing steam, which he again protected by a covering of wood, or other matters which conduct heat slowly.

"*There was no need of calling in the assistance of James Watt,*" (p. 256.) We cannot think that these were such trivial matters as they may appear to one conversant with the present state of civil-engineering; for be it remembered, that they were the performance of a young and self-taught engineer, at a period when such operations were only beginning to be carried on by a Brindley and a Smeaton.

"*The celebrated Burke,*" (p. 258.) Mr Burke's opposition is believed to have arisen, not from any hostility to Mr Watt or his patent, but simply from a sense of duty in defending what he conceived, or what were represented to him to be, the claims of a constituent.

"*Boulton and Watt received the third part of the value,*" &c., (p. 258.) *Stipulated* to receive, but, in fact, did not receive nearly that proportion.

"*The solution which he gave,*" &c., (p. 261.) Mr Watt's own words are, "It occurred to him, that if some mechanism could be devised moving upon centres, which would keep the piston-rods perpendicular, both in pushing and pulling, a smoother motion would be attained, and, in all probability, the parts would be less subject to wear."

"*The principle of the expansion of steam,*" &c., (Note, p. 263.) The following is the passage in the letter to Dr Small, referred to by M. Arago. The letter is dated 28th May 1769.

"I mentioned to you a method of still doubling the effect of the steam, and that tolerably easy, by using the power of steam rushing into a vacuum, at present lost. This would do little more than double the effect, but it would too much enlarge the vessels to use it all. It is peculiarly applicable to wheel-engines, and may supply the want of a condenser where force of steam only is used; for, open one of the steam valves, and admit steam until one-fourth of the distance between it and the next valve is filled with steam; shut the valve, and the steam will continue to expand, and to press round the wheel with a diminishing power, ending in one-fourth of its first exertion. The sum of the series you will find greater than one-half, though only one-fourth steam was used. The power will indeed be unequal, but this can be remedied *by a fly*, or several other ways." See Edin. Review for January 1809, p. 320.

For a full account of Mr Watt's Expansion Engine, and of his patent of 1782, see Robison, *Steam-Engine*, pp. 126 to 131, and still more particularly, Farey, "Treatise on the Steam-Engine," pp. 339 to 352, where extracts from the specification are given.

(Note on p. 264.) We claim also for Mr Watt the first application of the crank to engines producing rotatory motions. (See Edin. Review for Jan. 1809, p. 321, and Robison, *Steam-Engine*, p. 134.) And to him we are indebted for the beautiful contrivance of the sun and planet wheels, now little used, but of which we refer our readers to an excellent instance in the steam-engine of the brewery of Messrs Whitbread and Co. in London, where it has been in successful operation for upwards of fifty years. And there are many others.

"*The copying-press.*" (p. 266.) For a copy of Mr Watt's specification of this patent, with engravings of the machines, see the "Repertory of Arts and Manufactures," vol. i. 1794, p. 13.

"*Heating by Steam.*" (p. 266.) The most detailed account of Mr Watt's proceedings in the application of this most useful principle, is to be found in the preface to Buchanan on "the Economy of Heat and Management of Fuel."

"*Watt has been forgotten.*" (p. 267.) This is not quite correct in point of fact. Mr Watt's claims are accurately set forth in an able and impartial article on *Water*, in the edition of the Encyclopædia Britannica published in the year 1797. There is a very short account of the matter in Murray's Chemistry, ed. 1806, vol. ii. p. 158; and a most imperfect one in Nicholson's Chemical Dictionary, first edition, 1795, article *Water*, and Thomson's Chemistry, second edition, 1804, vol. i. p. 577, in all of which, however, the merit of the discovery is more or less attributed to Mr Watt.

But as far as the French chemists are concerned, M. Arago's statement is literally true. Fourcroy, in his voluminous work, "Système des Connaissances Chimiques," published in 1801, appears studiously to have avoided the very mention of Mr Watt's name, although he could not but be acquainted with his paper in the Philosophical Transactions for 1784, and had seen him at Paris in 1787, in the society of his friends Berthollet, Laplace, Monge, and Lavoisier, by all of whom Mr Watt's merits were appreciated. Cuvier, probably misled by this authority, gives the discovery to Cavendish and Monge, at p. 57 of his "Rapport Historique sur les Progrès des Sciences Naturelles," which was presented to Napoleon by the Institute in 1808, as well as in his Eloge of Fourcroy, read 1811, and of Cavendish, read 1812.

We take this opportunity of noticing that the writer of the life of Watt in Brewster's Encyclopædia, so late as 1830, has made assertions favourable to Cavendish's claims, founded on a perusal of the papers he has left. His Grace the Duke of Devonshire, as the representative of Mr Cavendish, granted permission to examine these papers. This was done by those eminent chemical philosophers, Mr Charles Hatchett and Mr Brande, who found nothing in them confirmatory of Mr Cavendish's claims to the priority of the theory, which rests, as before, on the published papers of Mr Cavendish, Mr Watt, and Sir C. Blagden.

Warltire (p. 269.) M. Arago has omitted to state, that Mr Warltire, in his letter dated Birmingham 17th April 1781, after relating his own experiments in the metal globe, goes on to say, "I have fired air in glass vessels since I saw you" [Dr Priestley] "venture to do it, and have observed, *as you did*, that though the glass was clean and dry before, yet after firing the air it became dewy, and was lined with a sooty substance." This proves Dr Priestley to have first made the experiment in glass vessels, as well as to have first noticed the dewy deposit.

"*Letter from Watt to Deluc.*"—(p. 270.) See Phil. Trans. vol. lxxiv. p. 330, and particularly Mr Watt's note at the foot of that page. The note is as follows:—"This letter Dr Priestley received at London, and after shewing it to several members of the Royal Society, he delivered it to Sir Joseph Banks the President, with a request that it might be read at some of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr Priestley's new experiments, begged that the reading might be delayed. The letter, therefore, was reserved until the 22d of April last [1784], when, at the author's request, it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost *verbatim*, in this letter to M. Deluc; but, to authenticate the date of the author's ideas, the parts of it which are contained in the present letter are marked with double commas."

"*Une quantité d'eau très sensible.*"—(p. 271.) Lavoisier's paper, in which these words occur, appears at p. 472 of that volume of the "Mémoires de l'Académie des Sciences," which is entitled "Pour l'Année 1781," but which was not printed till the year 1784. The paper was read on the 11th of November 1783.

"*About the same time,*" *&c.*—(p. 272.) See Blagden's paper in Crell's Journal, vol. i. 1786, p. 59. It is, on many accounts, a very remarkable one.

"*Des jaloux.*"—(p. 274.) The words in the original letter are these: "Je vous conseillerai presque, attendu votre position, de tirer de vos decouvertes des consequences pratiques pour votre fortune. Il vous faut eviter de vous faire des jaloux."

Priestley's Letter of 29th April 1783.—(p. 276.) Mr Watt, in his reply to the above letter, uses these forcible expressions: "I deny that your experiment ruins my hypothesis. It is not founded on so brittle a basis as an earthen retort, nor on its converting water into air. I founded it on the other facts, and was obliged to stretch it a good deal before it would fit this experiment. * * * I maintain my hypothesis until it shall be shewn that the water formed after the explosion of the pure and inflammable air has some other origin."

"*Of which it appeared that the profits must be very great.*"—(p. 276.) This, no doubt, is the point of view in which it would strike abstract men of science, such as Berthollet and Arago. But in this manufacturing country, we well know that the novelty and ingenuity of a process are not of themselves sufficient to ensure a beneficial result; and, indeed, in the case of the very process in question, it happened that the first manufacturers who attempted to carry it into effect on a large scale, were ruined by it.

"*Of this marriage there were born four children, two sons and two daughters.*"—(p. 278.) Two of these died in infancy; a daughter married Mr Miller of Glasgow, and has left issue. The present Mr James Watt is the gentleman mentioned by M. Arago in various parts of this Eloge.

"*The noble features of the old man.*"—(p. 284.) The thoughtful cast of his features was so remarkable, as to draw from his friend Mr Richard Sharp, of conversational fame, the playful observation, "I never look at Mr Watt's countenance without fancying I behold the personification of abstract thought."

"*The meeting which agreed to the erection of the Statue in Westminster Abbey.*"—(p. 292.) This meeting was held at Freemasons' Hall, on the 18th June 1824. Besides those quoted by M. Arago, the speeches delivered on the same occasion by Lord Brougham, Lord Hatherton (then Mr Littleton), Sir Robert Peel, the Earl of Aberdeen, Mr Frankland Lewis, Mr Wedgewood, and Mr Wilberforce, are not less distinguished for the testimony which they bear to the merits of Mr Watt, than for their own dignified and touching eloquence. And it is at once most interesting and most gratifying to find among the list of eminent persons by whom this great national tribute was so ably supported, the name of the present Mr Boulton, long the associate and intimate friend of Mr Watt.

Historical Account of the Discovery of the Composition of Water.

By the Right Hon. Henry Lord Brougham, F. R. S., and Member of the National Institute of France.

There can be no doubt whatever that the experiment of Mr Warltire,[*] related in Dr Priestley's 5th volume, gave rise to this inquiry, at least in England; Mr Cavendish expressly refers to it, as having set him upon making his experiments.—(Phil. Trans. 1784, p. 126.) The experiment of Mr Warltire consisted in firing by electricity a mixture of inflammable and common air in a close vessel, and two things were said to be observed; *first,* A sensible loss of weight; *second,* A dewy deposit on the sides of the vessel.

Mr Watt, in a note to p. 332 of his paper, Phil. Trans. 1784, inadvertently states, that the dewy deposit was first observed by Mr Cavendish;

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[*] Mr Warltire's letter is dated Birmingham, 18th April 1781, and was published by Dr Priestley in the Appendix to the 2d Vol. of his "Experiments and Observations relating to various branches of Natural Philosophy; with a continuation of the Observations on Air,"—forming in fact the 5th volume of his "Experiments and Observations on different kinds of Air," printed at Birmingham in 1781.

Mr Warltire's first experiments were made in a copper ball or flask, which held three wine pints, the weight 14 oz.; and his object was to determine "whether heat is heavy or not." After stating his mode of mixing the airs, and of adjusting the balance, he says he "always accurately balanced the flask of common air, then found the difference of weight after the inflammable air was introduced, that he might be certain he had confined the proper proportion of each. The electric spark having passed through them, the flask became hot and was cooled by exposing it to the common air of the room: it was then hung up again to the balance, and a loss of weight was always found, but not constantly the same; upon an average it was two grains."

He goes on to say, "I have fired air in glass vessels, since I saw you (Dr Priestley) venture to do it, and I have observed, *as you did,* that, though the glass was clean and dry before, yet, after firing the air, it became dewy, and was lined with a sooty substance."

As you are upon a nice balancing of claims, ought not Dr Priestley to have the credit of first noticing the dew?

In some remarks which follow by Dr Priestley, he confirms the loss of weight, and adds, "I do not think, however, that so very bold an opinion, as that of the latent heat of bodies contributing to their weight, should be received without more experiments, and made upon a still larger scale. If it be confirmed, it will no doubt be thought to be a fact of a very remarkable nature, and will do the greatest honour to the sagacity of Mr Warltire. I must add, that the moment he saw the moisture on the inside of the close glass vessel in which I afterwards fired the inflammable air, he said, that it confirmed an opinion he had long entertained, viz. that common air deposits its moisture when it is dephlogisticated."

It seems evident, that neither Mr Warltire, nor Dr Priestley, attributed the dew to any thing else than a mechanical deposit of the moisture suspended in common air.—(Note by Mr James Watt.)

but Mr Cavendish himself, p. 127, expressly states Mr Warltire to have observed it, and cites Dr Priestley's 5th volume.

Mr Cavendish himself could find no loss of weight, and he says that Dr Priestley had also tried the experiment, and found none.* But Mr Cavendish found there was always a dewy deposit, without any sooty matter. The result of many trials was, that common air and inflammable air being burnt together, in the proportion of 1000 measures of the former to 423 of the latter, " about one-fifth of the common air, and nearly all the inflammable air, lose their elasticity, and *are condensed into the dew* which lines the glass." He examined the dew, and found it to be pure water. He therefore concludes, that " almost all the inflammable air, and about one-sixth of the common air, are turned into pure water."

Mr Cavendish then burned in the same way dephlogisticated and inflammable airs (oxygen and hydrogen gases), and the deposit was always more or less acidulous, accordingly as the air burnt with the inflammable air was more or less phlogisticated. The acid was found to be nitrous. Mr Cavendish states, that " almost the whole of the inflammable and dephlogisticated air *is converted into pure water.*" And, again, that " if these airs could be obtained perfectly pure, the whole would be condensed." And he accounts for common air and inflammable air when burnt together not producing acid, by supposing that the heat produced is not sufficient. He then says that these experiments, with the exception of what relates to the acid, were made in the summer of 1781, and mentioned to Dr Priestley, and adds, that " a friend of his (Mr Cavendish's) last summer (that is 1783) gave some account of them to Mr Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of its phlogiston; but at that time so far was Mr Lavoisier from thinking any such opinion warranted, that till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water." The friend is known to have been Dr, afterwards Sir Charles Blagden; and it is a remarkable circumstance, that this passage of Mr Cavendish's paper appears not to have been in it when originally presented to the Royal Society, for the paper is apparently in Mr Cavendish's hand, and the paragraph p. 134, 135, is not found in it, but is added to it, and directed to be inserted in that place. It is moreover not in Mr Cavendish's hand, but in Sir Charles Blagden's, and indeed the latter must have given him the information as to Mr Lavoisier, with whom it is not said that Mr Cavendish had any correspondence. The paper itself was read 15th January 1784. The volume was published about six months afterwards.

Mr Lavoisier's memoir (in the Mem. of the Académie des Sciences for 1784) had been read partly in November and December 1783, and additions were afterwards made to it. It was published in 1784. It contained Mr Lavoisier's account of his experiments in June 1783, at which, he says, Sir Charles Blagden was present; and it states that he told Mr Lavoisier of Mr Cavendish having "already burnt inflammable air in close vessels, and obtained a very sensible quantity of water." But he, Mr Lavoisier, says nothing of Sir Charles Blagden having also mentioned Mr Cavendish's conclusion from the experiment. He expressly states, that the weight of the water was equal to that of the two airs burnt, unless the heat and light which escape are ponderable, which he holds them not to be. His account, therefore, is not reconcilable with Sir Charles Blagden's, and the latter was most probably written as a contradiction of it, after Mr Cavendish's paper had been read, and when the Mémoires of the Académie were received in this country. These Mémoires were published in 1784, and could not certainly have arrived when Mr Cavendish's paper was written, nor when it was read to the Royal Society.

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* Mr Cavendish's note, p. 127, would seem to imply this; but I have not found in any of Dr Priestley's papers that he has said so.—NOTE BY MR JAMES WATT.

But it is further to be remarked, that the passage of Mr Cavendish's paper in Sir Charles Blagden's handwriting, only mentions the experiments having been communicated to Dr Priestley; they were made, says the passage, in 1781, and communicated to Dr Priestley, it is not said when, nor is it said that "the conclusions drawn from them," and which Sir Charles Blagden says he communicated to Mr Lavoisier in summer 1783, were ever communicated to Dr Priestley; and Dr Priestley in his paper (referred to in Mr Cavendish's), which was read June 1783, and written before April of that year, says nothing of Mr Cavendish's theory, though he mentions his experiment.

Several propositions then are proved by this statement.

First, That Mr Cavendish in his paper, read 15th January 1784, relates the capital experiment of burning oxygen and hydrogen gases in a close vessel, and finding pure water to be the produce of the combustion.

Secondly, That in the same paper, he drew from this experiment the conclusion, that the two gases were converted or turned into water.

Thirdly, That Sir Charles Blagden inserted in the same paper, with Mr Cavendish's consent, a statement that the experiment had first been made by Mr Cavendish in summer 1781, and mentioned to Dr Priestley, though it is not said when, nor is it said that any conclusion was mentioned to Dr Priestley; nor is it said at what time Mr Cavendish first drew that conclusion. *A most material omission.*

Fourthly, That, in the addition made to the paper by Sir Charles Blagden, the conclusion of Mr Cavendish is stated to be, that oxygen gas is water deprived of phlogiston; this addition having been made after Mr Lavoisier's memoir arrived in England.

It may further be observed, that in another addition to the paper, which is in Mr Cavendish's handwriting, and which was certainly made after Mr Lavoisier's memoir had arrived, Mr Cavendish for the first time distinctly states, as upon Mr Lavoisier's hypothesis, that water consists of hydrogen united to oxygen gas. There is no substantial difference perhaps between this and the conclusion stated to have been drawn by Mr Cavendish himself, that oxygen gas is water deprived of phlogiston, supposing phlogiston to be synonymous with hydrogen; but the former proposition is certainly the more distinct and unequivocal of the two: and it is to be observed that Mr Cavendish, in the original part of the paper, *i. e.* the part read January 1784, before the arrival of Lavoisier's, considers it more just to hold inflammable air to be phlogisticated water than pure phlogiston, (p. 140).

We are now to see what Mr Watt did, and the dates here become very material. It appears that he wrote a letter to Dr Priestley on 26th April 1783, in which he reasons on the experiment of burning the two gases in a close vessel, and draws the conclusion, "that water is composed of dephlogisticated air and phlogiston deprived of part of their latent heat."* The letter was received by Dr Priestley, and delivered to Sir Joseph Banks, with a request that it might be read to the Royal Society; but Mr Watt afterwards desired this to be delayed, in order that he might examine some new experiments of Dr Priestley, so that it was not read until the 22d April 1784. In the interval between the delivery of this letter to Dr Priestley and the reading of it, Mr Watt had addressed another letter to Mr De Luc, dated 26th November 1783,† with many further observations and reasonings, but almost the whole of the original letter is preserved in this, and is distinguished by inverted commas. One of the passages thus marked is that which has the important conclusion above mentioned; and that letter is stated in the subsequent one to have been communicated to several members of the Royal Society at the time of its reaching Dr Priestley, viz. April 1783.

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* It may with certainty be concluded from Mr Watt's private and unpublished letters, of which the copies taken by his copying-machine then recently invented, are preserved, that his theory of the composition of water was already formed in December 1782, and probably much earlier. Dr Priestley, in his paper of 21st April 1783, p. 416, states, that Mr Watt, prior to his (the Doctor's) experiments, had entertained the idea of the possibility of the conversion of water or steam into permanent air. And Mr Watt himself, in his paper, Phil. Trans. p. 335, asserts, that for many years he had entertained the opinion that air was a modification of water, and he enters at some length into the facts and reasoning upon which that deduction was founded. [NOTE BY MR JAMES WATT.]

† The letter was addressed to Mr J. A. De Luc, the well known Genevese philosopher, then a Fellow of the Royal Society, and Reader to Queen Charlotte. He was the friend of Mr Watt, who did not then belong to the Society. Mr De Luc, following the motions of the Court, was not always in London, and seldom attended the meetings of the Royal Society. He was not present when Mr Cavendish's paper of 15th January 1784 was read; but, hearing of it from Dr Blagden, he obtained a loan of it from Mr Cavendish, and writes to Mr Watt on the 1st March following, to apprise him of it, adding that he has perused it, and promising an analysis. In the postscript he states, "In short, they expound and prove your system word for word, and say nothing of you." The promised analysis is given in another letter of the 4th of the some month. Mr Watt replies on the 6th, with all the feelings which a conviction he had been ill treated was calculated to inspire, and makes use of those vivid expressions which M. Arago has quoted; he states his intention of being in London in the ensuing week, and his opinion, that the reading of his letter to the Royal Society will be the proper step to be taken. He accordingly went there, waited upon the President of the Royal Society, Sir Joseph Banks, was received with all the courtesy and just feeling which distinguished that most honourable man, and it was settled that both the letter to Dr Priestley of 26th

In Mr Cavendish's paper as at first read, no allusion is to be found to Mr Watt's theory. But in an addition made in Mr Cavendish's own hand, after Mr Watt's paper had been read, there is a reference to that theory (Phil. trans. 1784, p. 140), and Mr Cavendish's reasons are given for not encumbering his theory with that part of Mr Watt's which regards the evolution of latent heat. It is thus left somewhat doubtful, whether Mr Cavendish had ever seen the letter of April 1783, or whether he had only seen the paper (of 26th November 1783) of which that letter formed a part, and which was read 29th April 1784. That the first letter was for some time (two months, as appears from the papers of Mr Watt) in the hands of Sir Joseph Banks and other members of the Society during the preceding spring, is certain, from the statements in the Note to p. 330; and that Sir Charles Blagden, the Secretary, should not have seen it seems impossible, for Sir Joseph Banks must have delivered it to him at the time when it was intended to be read at one of the Society's meetings (Phil. Trans. p. 330, Note), and as the letter itself remains among the Society's Records in the same volume with the paper into which the greater part of it was introduced, it must have been in the custody of Sir C. Blagden. It is equally difficult to suppose, that the person who wrote the remarkable passage already referred to, respecting Mr Cavendish's conclusions having been communicated to Mr Lavoisier in the summer of 1783 (that is, in June), should not have mentioned to Mr Cavendish that Mr Watt had drawn the same conclusion in the spring of 1783 (that is, in April at the latest). For the conclusions are identical, with the single difference, that Mr Cavendish calls dephlogisticated air, water deprived of its phlogiston, and Mr Watt says, that water is composed of dephlogisticated air and phlogiston.

We may remark, there is the same uncertainty or vagueness introduced into Mr Watt's theory which we before observed in Mr Cavendish's, by the use of the term Phlogiston, without exactly defining it.* Mr Cavendish leaves it uncertain, whether or not he meant by Phlogiston sim-

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April 1783, and that to Mr De Luc of 26th November 1783, should be successively read. The former was done on the 22d, and the latter on the 29th April 1784. [NOTE BY MR JAMES WATT.]

* Mr Watt, in a note to his paper of 26th November 1783, p. 331, observes, " previous to Dr Priestley's making these experiments, Mr Kirwan had proved by very ingenious deductions from other facts, that inflammable air was in all probability the real phlogiston in an aerial form. These arguments were perfectly convincing to me, but it seems proper to rest that part of the argument on direct experiment." [NOTE BY MR JAMES WATT.]

ply inflammable air, and he inclines rather to call inflammable air, water united to phlogiston. Mr Watt says expressly, even in his later paper (of November 1783), and in a passage not to be found in the letter of April 1783, that he thinks that inflammable air contains a small quantity of water and much elementary heat. It must be admitted that such expressions as these on the part of both of those great men, betoken a certain hesitation respecting the theory of the composition of water. If they had ever formed to themselves the idea, that water is a compound of the two gases deprived of their latent heat,—that is, of the two gases, with the same distinctiveness which marks Mr Lavoisier's statement of the theory such obscurity and uncertainty would have been avoided.*

Several further propositions may now be stated, as the result of the facts regarding Mr Watt.

First, That there is no evidence of any person having reduced the theory of composition to writing, in a shape which now remains, so early as Mr Watt.

Secondly, That he states the theory, both in April and November 1783, in language somewhat more distinctly referring to composition, than Mr

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* Mr Watt, in his letter of 26th April 1783, thus expresses his theory and conclusions (Phil. Trans. p. 333): "Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red hot, and, upon cooling, totally disappear. When the vessel is cooled, a quantity of water is found in it, equal to the weight of the air employed. This water is then the only remaining product of the process, and *water*, *light*, and *heat*, are all the products" (unless, he adds in the paper of November, there be some other matter set free, which escapes our senses). "*Are we not then authorised to conclude, that water is composed of dephlogisticated air and phlogiston, deprived of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston, and united to elementary heat and light; that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?*"

Is this not as clear, precise, and intelligible, as the conclusions of Mr Lavoisier?—[NOTE BY MR JAMES WATT.]

The obscurity with which Lord Brougham charges the theoretical conceptions of Watt and Cavendish does not appear to me well founded. In 1784, the preparation of two permanent and very dissimilar gases was known. Some called these gases, pure air and inflammable air; others, dephlogisticated air and phlogiston; and lastly, others, oxygen and hydrogen. By combining dephlogisticated air and phlogiston, water was produced equal in weight to that of the two gases. Water thenceforward was no longer a simple body, but a compound of dephlogisticated air and of phlogiston. The chemist who drew that conclusion, might have erroneous ideas as to the intimate nature of phlogiston, without throwing any uncertainty upon the merit of his first discovery. Even at this day, have we *mathematically demonstrated*, that hydrogen (or phlogiston) is an elementary body; or that it is not, as Watt and Cavendish supposed at the time, the combination of a radical and of a little water? [NOTE BY M. ARAGO.]

Cavendish does in 1784, and that his reference to the evolution of latent heat renders it more distinct than Mr Cavendish's.

Thirdly, That there is no proof, nor even any assertion, of Mr Cavendish's theory (what Sir C. Blagden calls his conclusion) having been communicated to Dr Priestley before Mr Watt stated his theory in 1783, still less of Mr Watt having heard of it, while his whole letter shews that he never had been aware of it, either from Dr Priestley, or from any other quarter.

Fourthly, That Mr Watt's theory was well known among the members of the Society some months before Mr Cavendish's statement appears to have been reduced into writing, and eight months before it was presented to the Society. We may, indeed, go farther, and affirm, as another deduction from the facts and dates, that, as far as the evidence goes, there is proof of Mr Watt having first drawn the conclusion, at least that no proof exists of any one having drawn it so early as he is proved to have done.

Lastly, That a reluctance to give up the doctrine of Phlogiston, a kind of timidity on the score of that long-established and deeply-rooted opinion, prevented both Mr Watt and Mr Cavendish from doing full justice to their own theory, while Mr Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection or consistency.*

All three may have made the important step nearly at the same time, and unknown to each other; the step, namely, of concluding from the experiment, that the two gases entered into combination, and that water was the result; for this, with more or less of distinctness, is the inference which all three drew.

But there is the statement of Sir Charles Blagden to shew that Mr Lavoisier had heard of Mr Cavendish's drawing this inference before his (Mr Lavoisier's) capital experiment was made;† and it appears that Mr Lavoisier, after Sir C. Blagden's statement had been embodied in Mr Cavendish's paper and made public, never gave any contradiction to it in any of his subsequent memoirs which are to be found in the Mémoires de l'Académie, though his own account of that experiment, and of what then passed, is inconsistent with Sir Charles Blagden's statement.*

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* It could scarcely be expected that Mr Watt, writing and publishing for the first time, amid the distractions of a large manufacturing concern, and of extensive commercial affairs, could compete with the eloquent and practised pen of so great a writer as Lavoisier; but it seems to me, who am certainly no impartial judge, that the summing up of his theory (p. 333 of his paper), here quoted p. 321, is equally luminous and well expressed as are the conclusions of the illustrious French chemist. [NOTE BY MR JAMES WATT.]

† In the letter which Sir Charles Blagden addressed to Professor Crell, and which appeared in Crell's *Annalen* for 1786, professing to give a detailed history of the discovery, he says expressly, that he had communicated to Lavoisier the conclusions both of Cavendish *and Watt.* This last name appears in that letter for the first time in the recital of the verbal communications of the Secretary of the Royal Society, and is never mentioned by Lavoisier. [NOTE BY MR JAMES WATT.]

But, there is not any assertion at all even from Sir C. Blagden, zealous for Mr Cavendish's priority as he was, that Mr Watt had ever heard of Mr Cavendish's theory before he formed his own.

Whether or not, Mr Cavendish had heard of Mr Watt's theory previous to drawing his conclusions, appears more doubtful. The supposition that he had so heard, rests on the improbability of his (Sir Charles Blagden's), and many others knowing what Mr Watt had done, and not communicating it to Mr Cavendish, and on the omission of any assertion in Mr Cavendish's paper, even in the part written by Sir C. Blagden with the view of claiming priority as against Mr Lavoisier, that Mr Cavendish had drawn his conclusion before April 1783, although, in one of the additions to that paper, reference is made to Mr Watt's theory.

As great obscurity hangs over the material question at what time Mr Cavendish first drew the conclusion from his experiment, it may be as well to examine what that great man's habit was in communicating his discoveries to the Royal Society.

A Committee of the Royal Society, with Mr Gilpin the clerk, made a series of experiments on the formation of nitrous acid, under Mr Cavendish's direction, and to satisfy those who had doubted his theory of its composition, first given accidentally in the paper of January 1784, and afterwards more fully in another paper, June 1785. Those experiments occupied from the 6th December 1787 to 19th March 1788, and Mr Cavendish's paper upon them was read 17th April 1788. It was therefore written and printed within a month of the experiment being concluded.

Mr Kirwan answered Mr Cavendish's paper (of 15th January 1784) on water, in one which was read 5th February 1784, and Mr Cavendish replied in a paper read 4th March 1784.

Mr Cavendish's experiments on the density of the earth, were made from the 5th August 1797 to the 27th May 1798. The paper upon that subject was read 27th June 1798.

The account of the eudiometer was communicated at apparently a greater interval; at least the only time mentioned in the account of the experiments is the latter half of 1781, and the paper was read January 1783. It is, however, probable from the nature of the subject, that he made further trials during the year 1782.

That Mr Watt formed this theory during the few months or weeks immediately preceding April 1783 seems probable.† It is certain that he considered the theory as his own, and makes no reference to any previous communication from any one upon the subject, nor of having ever heard of Mr Cavendish drawing the same conclusion.

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* Could Blagden's letter to Crell also have escaped Lavoisier's notice? [NOTE BY MR JAMES WATT.]

† That the idea existed in his mind previously, is proved by his declarations to Dr Priestley, cited by the latter; by his own assertions, p. 335 of his paper, and by the existing copies of his letters in 1782. [NOTE BY MR JAMES WATT.]

The improbability must also be admitted to be extreme, of Sir Charles Blagden ever having heard of Mr Cavendish's theory prior to the date of Mr Watt's letter, and not mentioning that circumstance in the insertion which he made in Mr Cavendish's paper.

It deserves to be farther mentioned, that Mr Watt left the correction of the press, and everything relating to the publishing of his paper, to Sir Charles Blagden. A letter remains from him to that effect written to Sir Charles Blagden, and Mr Watt never saw the paper until it was printed.*

* The notes of Mr James Watt formed part of the manuscript transmitted to me by Lord Brougham, and it is at the express desire of our illustrious fellow-member, that I have printed them as a useful commentary upon his essay. [Note by M. Arago.]

History of the Sternoptixinæ, *a family of the Osseous Fishes, and their anatomical peculiarities; with a description of the* Sternoptix Celebes, *a species not hitherto noticed.* By P. D. Handyside, M.D., F.R.S.E., Lecturer on Anatomy. Illustrated by two Engravings. Communicated by the Author.

Sect. I. *Historical Remarks.*

Professor Hermann of Strasburg, about the year 1774 applied the name of *Sternoptix* (from the apparently plicated arrangements of the external covering in the sternal region) to a very rare osseous fish from around the West India islands, small in size, truncated in front, narrow and tapering behind, high-backed, very compressed, and presenting a triangular distinctly pellucid compartment of the caudal region just behind the anus.* From the last-mentioned character, as well as to distinguish it from another genus, from the Azores, since described by M. Olfers, under the name of *Sternoptix Olfersii,*† in which that character is wanting, the former is known to naturalists as the *Sternoptix diaphana.* Owing to the great rarity of this fish, it being hitherto known to no author excepting through the means of the very incorrect representation of it afforded by Hermann, and the specimen which that naturalist has left in the museum at Strasburg, no opportunity has yet offered for rectifying the mistakes into which Hermann fell, who described this fish as devoid of gill-membrane and of a lateral line, and who placed it among the *Apodes* of Linnæus, thus concluding it to be destitute of ventral fins. *Linnæus,*[*] indeed, at the conclusion of the description of his 1st class of fishes,—the *pisces apodes*—has, like Hermann, given very erroneously the characters of the *Sternoptix diaphana,* and it remained for *Cuvier,*[†] while noticing very shortly both species, to take occasion to correct some of the inaccuracies of *Hermann,* and from examination of the specimen which Hermann himself described, and which he saw at Strasburg, to introduce it at the end of the *Salmonidæ,* the fourth family of his order *Malacopterygii abdominales.*

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* See Der Naturforscher, Halle, Fasc. xvi. pl. 8. tom. i. figs 1, 2, copied by Walbaum of Lubeck in his edition of Artèdi's Ichthyologia (entitled Artedius renovatus) tom. iii. pl. 1. fig. 2., and by Cuvier in his Règne Animal, edit. 1829, pl. xiii. fig. 1., and by Shaw in his Zoology, vol. iv. p. 112.

† See Cuvier's Règne Animal, pl. xiii. fig. 2.

While the two species adverted to, offer, in respect to their general form, translucency in the caudal region, number of branchial rays, character of the teeth, &c., points of difference such as to have induced Cuvier to remark, "ces deux espèces pourront former un jour les types de deux genres," they yet agree in the situation of the teeth, and in some striking peculiarities of general structure and form.

But the species of *Sternoptix* which I proceed to describe differs from both of the preceding in many, even *generic* characters, such as the situation, character, and number of the teeth, the number of the branchial rays, and the components of the different fins, and it differs also in respect to the locality in which it occurs; for, while the two former seem to be confined to the West India Islands, and the warmer parts of the Atlantic, the latter has been hitherto observed only in the Eastern Archipelago. I am unwilling, however, in the present imperfect state of our knowledge respecting this family of fishes, to submit the species under consideration as a new genus, although (as we have shewn) the characters peculiar to it might justify such a course, and rather, as the species of the family of Sternoptix which we now know, do not exceed three, I propose to confine myself at present to attempting to supply what is manifestly a desideratum in the natural history of this tribe of fishes—a description of the anatomical peculiarities of a family of fishes, which, from its extreme rarity, has been left hitherto unnoticed, adopting in my description that subdivision of it into two genera, which Cuvier anticipated, we have seen, would eventually be found justifiable.

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[*] Systema Naturæ Gmel. p. 1150.

[†] Regne Animal, edit. 1829, tom. ii. p. 315.

Selecting, then, the character of presence or absence of pellucidity in the caudal region, and making the distinction to depend on that,—a character indeed which serves accurately to distinguish the two already known genera of this family, I shall, from the existence of pellucidity in the example before us, classify it with Hermann's species, naming it simply from its locality. Thus, adopting Cuvier's classification of fishes, we find the position occupied by the *Sternoptix Celebes* in the following analysis :—

OSSEOUS FISHES.
- SUBDIV. MALACOPTERYGII ABDOMINALES.
  - Fam. IV. (*a*) SALMONIDÆ.
  - (Fam. V.?) (*b*) STERNOPTIXINÆ.
    - *Gen.* I. Sternoptix diaphana.
      - *Species* α, S. Hermannii.
      - β, S. Celebes.
    - *Gen.* 2. Sternoptix Olfersii.

Sect. II. *On the Sternoptixinæ Family in general.*

LOCALITY. The Sternoptix is a small osseous fish, of a very remarkable form, and of rare occurrence, confined apparently to the East and West India islands, and the warmer parts of the Atlantic.

SIZE. When full grown, it does not exceed from two to three inches in *length*, exclusive of the tail; its *height*, exclusive of the fins, is almost equal to its length; while in *thickness* it does not exceed three-quarters of an inch, that is, about one-third of its height.*

FORM. Its *body* is high, and greatly compressed, supported laterally by long ribs, and, including the head and chest, is semicircular in form in front; and, while its general form is remarkably oblique, and more or less elliptical, it is a little elongated backwards to the point where its forked tail (which is about one-fourth of its length) begins. The *back* rises into a sharp ridge, which, at its middle, supports a single fin; behind this fin there is a small gibbosity or hump (membranous in the species of *Hermann*, and *Olfers*, muscular in that from *Celebes*), in a situation corresponding to the adipose and rudimental fin of the Salmonidæ, while, in front of the dorsal fin, the carinated fore part of the back after becoming sulcated, ends in two sharp ridges, which converge towards the upper part of the head, at the interparietal bone, and thence running downwards, finally diverge towards the nares at the middle of the upper lip.* The *trunk* presents inferiorly, between the extremities of the humerals and the crests of the pelvic bones, a sharp pellucid ridge or crest, a formation which depends upon the acute junction of the extremities of the ribs. On each side of this carinated ridge there is a row of from eight to twelve small depressed surfaces, corresponding, for the most part, to the extremities of the intercostal spaces, but which, having been regarded as folds of the textures around the sternum (a bone merely assumed to exist), suggested the name of *Sternoptix*. The *anus* is mean (*i. e.* equally placed between the head and tail). The *caudal region* is slightly convex, and terminates by an acute carinate edge, running along one-third of the length of the body. The outline of the *head* is, anteriorly, remarkably obtuse, and the height of that region is double its length. The *eyes* are large, salient, and naked; they occupy the middle third of the height of the head, and advance within a line of its anterior boundary. The *mouth*, which is directed upwards suddenly, descends very obliquely, so as to appear abrupt when viewed in front, and is therefore singularly capacious in the vertical direction; and the maxillary bones (the upper of which slides over the lower) form the superficial boundary of this opening. The *tongue* is small and rudimental. *Maxillary teeth* exist in the Sternoptix: these are very numerous and minute. They differ, however, in the several species, both in their form and arrangement, appearing either *en velours* (as in the S. Hermanni) or *en crochets* (as in the S. Olfersii and S. Celebes).

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* See Plate III. fig. 1.

The BRANCHIOSTEGOUS MEMBRANE is patent, though posteriorly its margin is retracted under the opercula, and in each of the semicircular interspaces of the branchial rays it exhibits a longitudinal depression or shallow sinus.

The BRANCHIAL RAYS are from five to nine in number, very slender, naked at the extremity, and they curve downwards, backwards, and inwards.

OPERCULES. The opercule is subarcuated, soft, fine, and elastic, and terminates inferiorly in a right angle, the anterior margin of which is parallel to the last branchial ray, and partly covers it. The two branchial apertures extend obliquely downwards underneath the lower jaw and branchiostegous membrane, nearly as far as the mesian line, where the narrow isthmus formed by the junction of the lingual bones to the symphysis of the humerals separates the two branchial openings from below. The *pre-opercule* has its shaft flattened, its border finely denticulated, and its angle armed with a strong spine projecting downwards and forwards. The *sub-opercule* is in form an incurvated cone, having its rounded base below, its concave thick margin in front, its outer surface scabrous, and studded with polygonal cells, its convex posterior border much attenuated, while superiorly it is acuminated.

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* See Plate III. fig 2.

SPINES. [2 Pre-opercular, 2 humeral, 2 ventral, and 1 dorsal.] Besides the spine of the *pre-opercule* already described, there is behind the trochlea of the humerus a small thick *spine* curving downwards and forwards, and overlapping the ulna and radius; and below the symphysis and isthmus of the ossa humeri, which are remarkably elongated at an acute angle, there exists a sharp *spine*, which terminates each bone, and projects downwards at a point midway between the centre of the mouth and the ventral fins. A fourth strong *spine* curves downwards and forwards from the extremity of the pelvic bones on each side, and immediately in front of the ventral fin; and, lastly, in front of the first dorsal ray there rises up obliquely backwards from the first interspinal bone an extremely strong, scaly, or membranous moveable *spine*, thick and grooved behind, but finely denticulated on its anterior margin.

FINS. The *dorsal fin* is short, and has from eight to ten soft rays, bifid at their extremities. The *pectoral fins* are small, and multi-radiated. They present respectively the same number of rays as the dorsal, the superior ray being double the length of the inferior, and the intermediate rays vary in length proportionally. The *ventral fins* possess each, besides the spine, from five to seven very slender rays. They are placed about the middle of the trunk, opposite to the dorsal fin, and immediately in front of the anus. The *anal fin* is shallow, with distant rays, and is nearly longitudinal (*i. e.* extends a considerable way from the vent towards the tail). It consists of thirteen distinct soft rays, each bifurcated at its extremity. The *caudal fin* is forked, and has from thirty-six to forty rays.

SURFACE. The surface of the body of the Sternoptix is naked (or devoid of scales). It is covered with a dark coloured translucent mucus or epithelion.

COLOUR. The colour is variegated and very brilliant when the fish is first caught. The entire surface of its body has a tin or dim silvery lustre. The back is of a dark olive-green colour: the spines are translucent, and partake of the amber tint of the iris; and the fins and tail are intermediate between an amber and a mottled vandyke-brown hue.

LATERAL LINE. The lateral line is smooth, solitary, superior, straight, and descending.

Sect. III. *The Sternoptix Celebes.*

The *peculiarities* of this fish, serving to distinguish it from the S. Hermannii, the other species of the same genus, I shall next enumerate.

Locality. The *Sternoptix Celebes* was caught by my friend Mr Thomas Kincaid, surgeon R. N. in September 1836, in the Straits of Macassar, 1° S. lat. and 119° E. long., and within thirty miles of the Celebes coast, during calm and clear weather. It was first observed swimming on the surface of the water, apparently disabled from a deep cut it had received upon the back. It is uncertain whether it frequents shoal or deep water, but some fish resembling it were observed swimming about the roots of trees which had been washed from the coast by the rains, and which trees the fish seemed to have accompanied from the coral reefs near the shore.

Size. The *length* of the specimen which I possess,* which appears to have reached full maturity, is, exclusive of the tail, two inches and a quarter, its *height* is two inches, its greatest *thickness* scarcely half an inch.

Form of Chest. On each side of the carinate and pellucid lower border of the chest, is a series of small foveæ or dimples, eight in number.† There is no sternum or osseous basis supporting these pits, but simply minute fibro-cartilaginous expansions which project from the extremities of the ribs, and unite with each other at the mesial line. Accordingly, the interspaces between these being muscular, there may hence ensue, during the relaxed state of that texture, the regular depressed appearance referred to.

The Caudal Region, which runs one-half of the length of the body, is convex and carinate below, and the posterior and lower triangular portion of this cavity is, from the vertebral spines downwards, resolved into a pellucid membrane.‡ This membrane, which consists of two layers of tegumentary texture, is sustained in a tense and vibratory condition *anteriorly* by four slender bony processes, which arise from the posterior margin of the anal interspinal bone (a bone which forms truly the posterior boundary of the abdomen), and after traversing the space between the laminæ of the pellucid membrane in which they radiate, in a direction obliquely downwards and backwards, serve finally to sustain the anterior rays of the anal fin. *Posteriorly*, this membrane is stretched upon the five last interspinal osseous spiculæ, which, in their turn, support the middle rays of the anal fin.§

Head. The ridge of the *principal frontal* and *interparietal* bones is distinctly dentated. There are also similar dentations on the inferior margin of the *sub-orbital* bone.

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* See Plate III. I am indebted for the very faithful and accurate delineations of the anatomy of the S. Celebes which illustrate this paper, to my estimable friend and late pupil, Mr Willington, surgeon at Saltisford, Warwick.

† Ibid. ‡ See Plates III. and IV. figs. 1. § Plate IV. fig 1.

The Mouth is set with maxillary and palatine teeth. The *maxillary teeth* are very numerous and minute, and they are arranged *en crochets*, three rows in each jaw.[*] Each tooth presents the form of two incurvated cones applied base to base, their concavities being directed towards the interior of the mouth.[†] The *palatine* teeth are much larger, and the existence of them appears to be distinctive of this species. They are five in number on each side of the mesian plane, and being arranged *en cardes*, they, on the approximation of the jaws, close after a dove-tailed manner.[‡]

The Branchial Arches[§] are four in number, on the posterior half of the first three of which are placed several slender and curved dental appendages, resembling the teeth of a garden rake. On the first or most anterior arch we count seven, on the next five, on the third three of these dental appendages. The superior half of each of these appendages is split down, or appears bristly along its distal half, and likewise at its extremity. Moreover, on the anterior half of these three arches are placed several tufts of short straight teeth, arranged *en brosses*. On the first arch are five, on the next four, and on the third three of these dental tufts. Superiorly, *the first and second pairs of arches*, after ascending in a curve, are attached by means of ligaments, situated beneath the cranium, to the sphenoid bone. The *third and fourth pairs* are attached to the upper borders of the œsophagus. Inferiorly, these arches are seen to join those of the opposite side, immediately below and in front of the large oval aperture of the œsophagus.[¶] Here they are retained in contact, partly by means of a strong fibro-cellular membrane or ligament of a triangular form,[‖] partly by the usual intermediate chain of three minute bones continued backwards from the lingual bone.

The Branchial Rays are five in number, naked, attenuated, and curved.[**]

The Dorsal Rays amount to (one moveable spine and) ten soft rays, each bifurcated at its extremity, the terminating points fimbriated.

The Anal Fin in the Sternoptix Celebes is furnished with soft rays, thirteen in number, connected to each other by a transverse band near the root, and bifid at their extremities, between the most anterior ray of which fin, and the posterior margin of the verge of the anus, there exists in this species (as a termination to the anal interspinal bone) a strong *anal spine*, bifid at its extremity, and curved forwards in a direction parallel to the ventral spine.[††] The large anal interspinal bone, joined to a great inferior spinous process, and extending down behind the anus, forms in this, as in a great number of other fishes, truly the posterior boundary of the abdomen.[‡‡]

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[*] Plate III. fig. 3, and IV. fig. 1.
[†] Plate III. fig. 4.
[‡] Plate III. fig. 2, and IV. fig. 3.
[§] Plates III. fig. 2; IV. figs. 2 and 3.
[¶] Plate III. fig. 3.
[‖] Ibid.
[**] Plates III. and IV. fig. 1.
[††] Plates III. and IV. fig. 1.
[‡‡] Ibid.

The Caudal Fin is attached, as in the salmonidæ, to a very fleshy root, being moved by very powerful muscles. It is forked, and consists of thirty-six flattened, articulated, firm, and highly elastic rays. These are separated by a deep cleft into two conical fasciculi, the lower one being deeper, stronger, and more elongated, while each is inserted into the free border of one of the two segments of the semilunar shaped terminal caudal vertebra.* The seven highest rays of the root of the tail arise together from the integuments over the three or four last superior spinous processes, while the four lowest rays proceed from a line near the apices of the two last inferior spinous processes.†

The Pectoral Fins in this species are each furnished with ten soft rays.

The Ventral Fins present each seven soft rays.

Edinburgh, 4 Surgeons' Square,
July 27. 1839.

* Plate IV. fig. 1. † Ibid.

Table of Mean Temperature of Orkney. Communicated by the Rev. Charles Clouston of Sandwick Manse.

The following Table shews the mean monthly and annual temperature from 1827 till 1838 inclusive, with the mean temperature of these twelve years, which may be considered the mean temperature of Orkney. The register was kept at the manses of Stromness and Sandwick, and marked twice a-day, at 10 o'clock A. M. and 10 P. M.

Years.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Ann.
1827,...	35°.09	35°.09	38°.59	43°.83	49°.16	53°.40	55°.61	55°.04	54°.33	50°.95	43°.11	43°.24	46°.45
1828,...	40.22	38.82	41.45	43.60	45.30	56.04	58.25	57.26	55.41	54.19	45.53	43.24	48.27
1829,...	36.14	39.46	40.69	41.51	49.99	53.43	57.12	54.36	50.21	45.56	41.40	39.51	45.78
1830,...	37.96	36.71	41.83	44.80	50.77	51.66	56.14	53.14	53.46	49.41	42.76	36.33	46.24
1831,...	36.70	37.71	42.65	44.88	48.70	56.13	57.86	58.90	54.73	51.73	40.50	44.07	47.88
1832,...	41.49	42.22	42.22	46.24	47.31	54.73	54.25	56.07	52.61	49.38	45.43	41.40	47.77
1833,...	37.14	38.37	38.68	43.23	51.43	51.60	54.73	52.40	52.34	49.20	41.58	39.28	45.83
1834....	38.90	40.21	41.11	43.05	48.75	58.02	58.03	56.89	52.90	48 07	44.01	45.17	17.92
1835,...	39.75	39.48	41.20	42.25	46.01	51.76	53.57	54.77	53.01	45.70	45.68	40.44	46.13
1836,...	39.14	37.46	39.61	41.39	47.77	52.03	52.51	51.83	48.35	45.91	40.93	38.57	44.62
1837,...	38.82	39.32	36.54	39.13	45.24	51.06	50.56	53.75	51.58	49.36	41.59	42.44	44.94
1838,...	33.56	31.31	38.64	39.23	44.75	48.20	53.86	52.28	50.28	45.77	39.71	41.78	43.28
Monthly Mean,	37.90	36.01	40.26	42.76	47.93	53.17	55.20	54.72	52.43	48.76	42.68	41.28	
												Mean of 12 years,...	46.25

Notice concerning an Improvement in the Construction of the Single Vision Prism of Calcareous Spar. By William Nicol, Esq. F.R.S.E. Communicated by the Author.

The single vision prism of calcareous spar which I invented several years ago, and which is described in this Journal, vol. vi. p. 83, and vol. xvi. p. 372, though well adapted for examining various phenomena depending on the polarization of light; yet, on account of its length being so much greater than its breadth or thickness, the field of view is less than could be wished. This imperfection may be greatly diminished by shortening the prism without altering its thickness. The shortening is accomplished by cutting off equal portions from each extremity; and when the prism is thus shortened to half its former length, the field of view will be found greatly enlarged.

To construct a prism, then, on the shortened principle, two rhomboids of spar must be procured, and if the shorter diagonals of their terminal planes be four-tenths of an inch, the length of the sides or edges may be five-tenths of an inch. One of the terminal planes of each must be inclined to the obtuse lateral edge at an angle of 68°, and polished. One-half of each rhomboid must then be removed by acting obliquely on the obtuse edge, in such a manner that the plane produced shall form an angle of 90° with the terminal plane. When these planes are duly polished, they are to be united by Canada balsam. The smaller extremity of each portion must then be rendered opaque, which may be done by pasting on it a piece of black tracing paper.

A prism of the above dimensions will be found well adapted for the general purposes of analysis, but one of a still smaller size will be found equally efficacious, viz. even when the shorter diagonals of the terminal planes are only three-fourths of an inch. When a shortened prism is to be applied to the eye-lens of a compound microscope, however, one of larger dimensions will be found to answer better. The one I have found to furnish the greatest field, has the shorter diagonal of the terminal planes five-tenths of an inch, and the length of the sides also five-tenths of an inch. With the exception of a very small segment towards the outer or blue edge of the extraordinary ray, the whole disc is visible, at least with a slight inclination of the eye. In placing the prism above the lens, some nicety is required, in order to obtain the greatest possible field of view. It requires to be inclined a little to the axis of the microscope. If the acute solid angle of the prism be farthest from the body of the observer, the inclination must be towards the body.

In constructing instruments of this kind, the greatest accuracy is necessary. The corresponding angles of the two portions must be equal. Both the terminal and sectional planes must be set on so as not to incline in the least perceptible degree more to one side than the other. When inaccuracies in these respects occur, the circular rings are sure to be deformed. It may, therefore, be of use to point out an easy method of determining whether the construction be accurate or not. At the distance of two or three yards, direct the prism to the crossing of the astragals of a window, make the prism revolve slightly backwards and forwards, and if no motion of the astragals ensue, the terminal planes may be considered as parallel. Incline it, then, until one of the astragals coincide with the junction of the two rays, and if there be no overlapping of the astragal, or if it appear as distinct and defined as when seen with the naked eye, the instrument may be considered as having been properly constructed.

On the Geographical Distribution of Insects. (Continued from p. 111.)

Stations.—When we observe the great number of localities in which certain species of insects are found, we might be tempted to believe that these animals have no well determined stations. It is true, that theirs are less fixed than those of vegetables, and that it would be difficult to assign a precise one to some species; but, in general, these are exceptions, which a little observation enables us to recognise as such.

Let us take, for example, a very natural genus, admitted by all entomologists without exception, and at the same time very rich in species, spread over all parts of the globe, and consequently subjected to external circumstances of the most varied kind. The genus *Cicindela* perfectly answers these conditions, and we shall see that, in the point of view we are now considering, it is divisible into many very distinct groups.

The first (including *C. Cayennensis*, *bipunctata*, *luridipes*, &c.) proper to intertropical America, lives in the forests on leaves, and never lights on the earth but accidentally. A single individual is never seen in open places nor by the margin of waters.

A second (*C. sylvatica*, *sylvicola*, &c.) frequents heaths, pathways, and clearings in the wood, without ever resting on leaves. A third (*C. germanica*, *gracilis*), lives only in cultivated fields, dry meadows, and similar places. A fourth (*C. maritima*, *tortuosa*, *trifascicata*), is met with only on the shores of the sea, and ascends the sides of rivers no farther than the influence of the tide is felt. While a fifth (*C. ventralis*, *apiata*, *melaleuca*) begins to appear where the preceding stops, never leaving the vicinity of fresh water.

These stations are strictly rigorous, and there is no doubt that if the habits of all the species of this genus were known, we could find grounds for indicating other divisions of the same nature.

Stations are characterized either by the insects which frequent them, or the dominating physical characters, according to the point of view in which we regard them; but the latter is the most convenient. We may, in this way, distinguish as many as botanists are in the habit of doing for plants. 1*st*, *The Sea.* No insect is known, which passes its whole life in this element, and only a very small number frequent it in the perfect state. Scarcely any one can be mentioned, indeed, except *Gyrinus marinus*, which likewise lives in fresh water, and the singular Hemiptera of the genus *Halobates*, which Eschscholtz found under the tropics running along the surface of salt water like the *Hydrometra*. 2*d*, *Shores of the Sea.* These are rather rich in species peculiar to them, especially in warm countries. It is principally in this situation we meet with the genus *Pimelia*, whose existence seems connected with that of the plants of the genus *Souda*. 3d, *Brackish waters*. These have also a small number of peculiar species, such as the *Hydræna marina*, which is not found in the sea, as its name would lead us to expect. According to Kirby and Spence, it is only in dried salt-marshes that we find certain Hemipterous species of the genus *Acanthia* (*A. saltatoria*, *littoralis*, *zosteræ*, &c.) 4*th*, *Fresh waters*. Among the species which inhabit these, some live completely immersed, and may be divided into such as continue there only in their earlier states (*Culicidæ*, *Libellulidæ*, *Phryganiæ*, *Ephemeræ*, &c.) and such as pass their whole life in them, *Hydrocantharidæ*, *Hydrophilus*, *Nepa*). The latter can leave their watery abodes for a short time (which they usually do at the approach of night), and thus they partake of the nature of aquatic, terrestrial, and aërial animals. The other aquatic species, although living in the water, are unable to swim, and are usually found clinging to aquatic plants. Such are the *Hydrænæ*, certain *Sphæridia*, and a few *Curculionidæ* of the genera *Bagous*, *Hydronomus*. The circumstance of waters being either stagnant or running, has an influence on the species inhabiting them. For example, the greater part of the Dytiscidæ frequent ponds and marshes, in preference to rivers, while the opposite is the case with *Gyrinus*, *Haliplus elevatus*, *Macronychus*, &c. 5*th*, *Margins of fresh waters*. The species met with in such situations, differ with the nature of the soil. If it be sandy, we then find *Omophron*, which dig into it a few inches beneath the surface. *Chlaenius* and *Bembidium* select what is stony and mingled with gravel; if it be muddy, it is adapted to *Elaphrus*, *Helophorus*, *Parnus*, &c.

6*th*, *The various kinds of soil*, which form an extensive catalogue, according as they are dry or moist, cultivated or uncultivated, rocky, sandy, compact, light, &c. have each their appropriate insects, which either inhabit the surface or the interior. The former are said to be *epigeous*, the latter *hypogeous*. 7*th*, *Mountains*. Their declivities afford numerous stations for insects. The species which live near the summits, are called *alpine*, such as inhabit their lower stages, *subalpine*. But these epithets are only applied with propriety when the species are not found, or at least are very rare in the neighbouring plains. 8*th*, *Living vegetables.* These may be considered under two points of view, as insulated individuals, or as assembled into forests, &c. Every part of a plant is liable to be attacked by particular insects, to which it serves as a station. We ought, consequently, to distinguish the roots, the stalk, the leaves, &c. Some authors call the insects which live in the interior of plants, *endophytous*, while those found on their exterior, whether they live at the expense of the plant or not, are named *epiphytous.*

Considered in their collected state, vegetables present differences no less considerable. Certain insects seem to delight only in extensive forests; others prefer coppices, gardens, meadows, &c. Each species is attracted to these different places, not only by the plants which serve it for food, but by certain conditions of heat, light, or humidity. Under the tropics, it is not in general in the depth of the virgin forests that the greatest number of insects is to be found. The perpetual shade which prevails there, induces a degree of coolness and moisture not congenial to most of these animals. They prefer the skirts of woods, or the clearings which appear among them at rare intervals.

9*th*, *Dead or decomposed vegetables.* A multitude of different kinds live in these, particularly in their early states. 10*th*, *Living animals.* Mammiferæ and birds are the only subjects, among the vertebrata, liable to be attacked by insect parasites. In the class Mollusca, the *Helex nemoralis* may be mentioned as being in a like predicament, for it is preyed on by the larva of Drilus flavescens. The rest of the animal kingdom has no relation with the insect tribes. Such of them as live on the vertebrata, are termed *epizoites.* 11*th*, *Dead animals.* The Coleoptera and Diptera are the only orders which make these their pabulum. 12*th*, *Animal rejections.* The rejectments of carnivorous animals are frequented by few insects; but those of herbivorous ones afford a station and nutriment to an entire family of Coleoptera, viz. the Coprophaga, and also to many other species of all the orders.

All these stations are subordinate, as far as relates to the perfect insects, to the period of the year at which the latter make their appearance; for, at all other times, they present almost a complete solitude, and these animals are found only in one or other of their three early states, that of egg, larva, or pupa.*

Period of the appearance of the perfect Insects. Every species, under whatsoever climate it may live, has one or more fixed periods for appearing in the perfect form, according to the rapidity with which its various transformations take place. These periods may either be accelerated or retarded by the effect of temperature; but they are not the less regular, when considered in a general manner.

In cold and temperate countries, they commence with the return of heat and vegetation, and a remarkable coincidence exists between the appearance of the insect and the plant which is to afford it food. Hence we may conclude, *a priori*, that wherever vegetation is developed with extreme rapidity, and as it were by a sudden impulse, the same thing will take place with insects, and *vice versa;* and this, in reality, is found to be the case. Thus, in the polar regions, where heat equal to that of the tropics all at once succeeds a degree of cold much more intense than that of our severest winter, while the ground is not yet free from snow it becomes covered with plants in flower, and the air filled with insects, not very varied in regard to species, but of which the individuals are in myriads. In proportion as we recede from these desolate regions, and reach more southern latitudes, vegetation and insects are found to be developed less suddenly, but the two always continuing cotemporaneous. In our temperate regions, the months of April, May, and June, are the seasons when these animals are in greatest abundance. The number diminishes during the heats of summer, and this decrease is the more perceptible the further south we advance. This fact is deserving of being remarked, for it accords with what takes place on a larger scale in intertropical regions. In September and October, a kind of augmentation again occurs, which coincides with the flowering of certain autumnal plants.

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* For more ample details regarding stations, the numerous works on the class of insects may be consulted, in which they are in general accurately indicated. A Memoir by M. G. Silbermann, inserted in the *Revue entomologique*, tom. i. deserves particularly to be mentioned.

On the arrival of winter, no inconsiderable numbers still subsist, concealed in their various retreats, but none are then disclosed, except a few, such as *Geometra brumaria*, which darts about our gardens even to the end of December; *Trichocera hyemalis*, which may be seen during the fine days of winter performing their aërial dances above the snow; *Boreas hyemalis*, certain *Poduræ*, and the *Chionea araneoides*, which are found only on the snow itself. Winter, in our climates, thus divides the year into two very distinct periods, and, by arresting vegetation, deprives insects of the means of existence.

In the equinoctial regions, where winter is unknown, and where the year is divided, more or less regularly, into two seasons—the wet and the dry—without vegetation being ever suspended, it might seem, at first view, that nearly an equal quantity of insects should be found during the whole year; but this is very far from being the case. The seasons, in this respect, are almost as determinate as in Europe. The dry season has nearly the same effect on insects that winter has with us; they disappear almost entirely, and are not again visible till the rains. On the other hand, the latter, if they reach a certain maximum, produce the same effect on them, so that, in reality, their seasons of abundance are often rather brief. This is the more obvious, the nearer we approach the equator.

Thus, in Guiana, the rainy season, which commences towards the end of November, brings forth a considerable number of insects, which diminish rapidly in proportion as they become more severe; insomuch, that very few are to be seen in January and February. In March, an interval of fine weather, which lasts for a month, and which the natives call the *March summer*, causes them to appear in tolerable abundance. From April to June, when the rains fall with such violence that the country is literally inundated, the forests flooded and shrouded in a cloud of vapour, insects completely disappear. Towards the close of June, when the fine weather commences, they again shew themselves, and increase with astonishing rapidity till the end of August. This month, and that of July, afford a richer harvest to the entomologist than all the rest together. But the dry season, which is then confirmed, reduces them considerably, and continues to do so more and more till the end of November, when, as has been stated, the rainy season resumes its sway.

At Rio Janeiro, situate exactly under the tropic of Capricorn, a different order prevails. Insects appear in September with the first rains; but, as the rainy season, though very severe, is far from equalling that of Cayenne* in intensity, they do not disappear on its attaining its maximum of violence in January and February; on the contrary, they go on increasing along with the rains, and these two months are the most prolific in the year. In April they diminish along with the rains, and during the dry season, from May to the end of August, scarcely any are to be found but Carabidæ and Melasomas, which have taken refuge under stones, bark, &c.

At Buenos Ayres, 35° south latitude, the seasons of the appearance of insects are regulated nearly in the same manner as in southern Europe, but in an inverse sense, on account of the difference of the hemisphere. Insects appear in September and October in spring; the powerful heats of midsummer (January and February) cause them to disappear, as with us; they again multiply in autumn (March and April); and, finally, from May to the middle of September, only a small number are met with.

Chili, situated under the same parallels as Buenos Ayres, differs from it, in the view we are now taking, only in this, that, rain being almost unknown from the middle of spring to the middle of autumn, it is only at the commencement of the former and the end of the latter that insects are common, while at Buenos Ayres they are found throughout the whole duration of these two seasons.

Our acquaintance with other warm countries of the globe, in regard to the subject of which we now treat, is limited to what we have learned from M. Westermann respecting the Cape of Good Hope, Bengal, and Java.† There, as in America, the condition of insects is in perfect accordance with the dry and rainy seasons; and as these two seasons act upon them through the medium of the plants which flourish during the second, and acquire a sombre hue during the first, it may be established as a general law, that throughout the globe the march of insects is in intimate relation with that of vegetation. It is, moreover, probable that the dryness acts in a direct manner on these animals, as it is known to do on the caymans of America, which it throws into a real state of hybernation, as has been shewn by M. de Humboldt, and as M. Lacordaire has himself witnessed.

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* The annual quantity of rain which falls in Cayenne, is about 130 inches, while it does not exceed 80 at Rio Janeiro.

† Germar's Magazin, tom. iv. p. 411.

It may be asked, if insects really perish during the dry season, or whether they only conceal themselves, as many of ours do throughout the winter. Our author is of opinion that the greater part perish, for they are seldom found under bark, in the interior of the earth, or of vegetables, which might be expected if the case were otherwise.

Another consideration, intimately connected with this, relates to the form under which insects pass the winter in our climates. All of them which appear in the spring must exist in the beginning of winter, and necessarily pass that season either in the state of egg, larva, pupa, or perfect insect.

The species which come under the first category are of small amount compared to the entire mass of insects, which is doubtless owing, on the one hand, to the circumstance, that the greater part of young larvæ, if disclosed in early spring, would fail to find food; and, on the other, to this, that the substances in or upon which certain eggs must be deposited, such as leaves or the larvæ of other insects, do not exist at that period. This refers chiefly to the kinds which produce many generations in a year, and to such as undergo an incomplete metamorphosis, which do not attain their full development till an advanced period of the year. In either case, the eggs, being laid very late, cannot be hatched without the young larvæ being exposed to the want of food, and it was consequently necessary that they should remain under that form throughout the unfavourable season. It has been remarked as a singular fact in regard to the Lepidoptera, that it is only such of them as have caterpillars which feed on perennial plants, that hybernate in the egg or larva state, while those whose caterpillars live on annual plants do so under the chrysalid form. The reason of this is, that the leaves of perennial plants appear earlier than those of annuals, so that the young caterpillars find them a ready food as soon as hatched, which could not be the case with those living on the other.

The majority of the species which hybernate as larvæ, is necessarily composed of those which live in that state for several years, such as *Melolontha vulgaris*, *Lucanus cervus*, the greater part of the Longicornes, many Elateridæ and Buprestidæ, Lepidoptera with endophytous larvæ, &c. The remainder consist of larvæ disclosed about the middle of autumn, and which have been surprised by the cold before accomplishing their change into a pupa.

Hybernating pupæ belong almost entirely to the lepidopterous order, and to the species which, as has been seen, live on annual plants. These chrysalides await disclosure till the flowers appear on which the butterfly feeds, so that, in general, they are not earlier than the kinds which passed the winter in the state of egg or larva.

Finally, a great number of perfect insects hybernate, and shew themselves not only in spring, but even during warm days in winter. The greater part of these are Coleoptera, and chiefly of a carnivorous or lignivorous kind, as both these may still find some alimentary substances at that season, although it is probable, considering the almost complete disappearance of their fatty tissue in spring, they require but very little nourishment in this interval. The principal cause of the hybernation of these species seems to be, that the sexual union has not taken place, the non-accomplishment of the generative functions having the greatest influence on the longevity of insects.

The order in which the various species appear on the return of spring is thus determined, in a great measure, at the close of the preceding autumn. During the remainder of the warm season it is regulated by the number of generations, and the period which each species requires to undergo its metamorphosis. These appearances coincide in general with the flowering of certain plants, so that it is possible to divide the year, as has been done by Kirby and Spence, into different periods, characterized by the simultaneous appearance of certain species of flowers and insects.*

Habitations.—This department of the subject, more important than any hitherto touched upon, divides itself into the four following branches :—*1st*, The determination of the number of species of insects existing on the globe. *2d*, The proportion according to which the various families are spread over different countries. *3d*, The extent of the habitation of species and groups. *4th*, The division of the surface of the globe into entomological regions, characterized by the insects which predominate in each of them.

In regard to the first subject of inquiry, it will appear obvious, that, in the present state of entomology, it is impossible to estimate the number of actually existing species, except by way of induction, that is to say, by setting out from a point better known. Plants, which have always been collected with greater ease than insects, and which have a most intimate relation to these animals, have properly been assumed as the point of departure by authors engaged in this calculation. By comparing the number of insects with that of the plants of a given country, we obtain the relation which subsists between these two classes of organized beings; and, by applying this relation to the total number of plants supposed to exist, we arrive at the approximate result sought.

Now there exist in France, according to the *Botanicon Gallicum* of MM. Decandolle and Duby, 7194 species of plants: it may now be considered to be 7400, with the addition of those discovered since the publication of that work. The number of insects in the same country, as far as it can be determined from the study of authors and the inspection of the richest collections, is not less than 15,000, which gives about two insects for each plant. This number will appear too low, for there are vegetables, such as the oak, which support twenty times that number; but if we reflect that the Cryptogamia, which supply food to a very small number of these animals, form the half of these 7400 plants above mentioned, and that the same insect often lives at the expense of several plants, it will be admitted, that the allowance of three insects for each plant is a reasonable proportion. It may be alleged that the proportion must be greater under the tropics, and this is probably the case with phytophagous species; but, on the other hand, there is a deficiency of creophagous kinds, which restores the equilibrium. Now by estimating, with M. Decandolle, the total number of existing vegetables at from 110,000 to 120,000, we obtain from 330,000 to 360,000 for that of insects.*

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* We have omitted M. Lacordaire's account of Kirby and Spence's views on this subject, as being comparatively familiar to an English reader, or at all events readily accessible to him in their *Introduction*, vol. iv. p. 512.

It is more difficult to determine how the above number ought to be divided among the different orders, for this calculation can only be founded on the species existing in collections; and it is well known that some departments have been much more assiduously cultivated than others. The following is the result of our author's inquiries on this subject.

Fifteen years ago, Mr Macleay estimated the amount of species preserved in collections at 100,000. This number was adopted by Latreille; but our author conceives that we should be much nearer the truth by lowering it, as has been done by Dr Burmeister, to 80,000. Of this number the Coleoptera must form nearly one-half; for very little short of 30,000 are contained in the Parisian cabinets alone, and many others must exist in different places, by the union of which, at least 40,000 would be attained. In making this calculation, we ought to keep in mind the unequal manner in which exotic species arrive in Europe. Thus, Paris and Berlin receive principally those of America; the species of the Sunda and Molucca islands go chiefly to Holland; England receives them from New Holland and Bengal more than from other countries, &c. The exchanges which entomologists make with each other remove but imperfectly this original inequality. It therefore follows that every country possesses, in some degree, peculiar exotic species; and consequently, we will arrive at a higher number by uniting the collections existing in all, than if we draw our inferences merely from the collections of any one country.

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* Kirby and Spence, by a similar calculation, make the number 400,000, which appears rather too high.

The remaining 40,000 species may be divided, according to M. Lacordaire, as follows :—Hymenoptera, 12,000 ; Lepidoptera, 10,000; Diptera, 10,000 ; Hemiptera, 5000 ; Neuroptera, 1500 ; Orthoptera, 1000 ; Parasita, 500.

On the supposition that we are acquainted with one-third of the Coleoptera, one-half of the Lepidoptera, one-fifth of the Hemiptera, one-sixth of the Hymenoptera, Neuroptera, and Orthoptera, one-tenth of the Diptera, and one-twentieth of the Parasita, we would obtain the following numbers as the absolute amount of the species existing on the globe :—

Coleoptera, ...	120,000
Diptera, ...	100,000
Hymenoptera, ...	72,000
Hemiptera, ...	25,000
Lepidoptera, ...	20,000
Parasita, ...	10,000
Neuroptera, ...	9,000
Orthoptera, ...	6,000
	362,000

With regard to the number of individuals of each species, or, what is the same thing, its degree of rarity, it will readily be perceived, that there are no data whereon to found a calculation likely to afford even an approximation to the fact. It may be merely remarked, that, with respect to the great majority, they are the more common as we approach certain countries, which are, as it were, the centre of their habitation ; and that, on leaving this point, they cease more or less abruptly at distances varying according to the direction we follow, so that they may be represented as radiating from the centre in question. This fact is of importance to be noted, for it is partly on it that the possibility rests of establishing entomological regions.

Absolute and Relative number of Species, Genera, and Families in different countries.—The absolute number of the species of a country depends on a multitude of circumstances which are for the most part the same as those enumerated above. It is evident that its richness, in this respect, will be proportionate to its extent, its temperature, the nature of its vegetation ; to the number and nature of its stations ; to the absence or presence of barriers separating it from neighbouring countries.

All these causes, moreover, may be so combined in a certain country, as reciprocally to compensate each other; while in another they may harmonize in such a way as to produce the highest possible result. It is thus that Africa and equatorial America, though nearly on an equality with respect to temperature, are so differently circumstanced in their insect productions; the latter being infinitely the richest, because it is generally more wooded and moist.

The most general result at which we can arrive, and which is particularly deserving of attention in examining this question, is that the number of species augments as we recede from the poles and approach the equator. Heat being the most important condition for vegetation, must also be so for insects. But it must not be thence concluded that this increase takes place equally in all countries. The vicinity of Paris, for example, is as rich in species as that of Marseilles, which is owing to its being more humid, and presenting more varied stations to these animals.

The law of which we speak, can only be demonstrated, with any degree of precision, in reference to the Coleoptera, and even in their case somewhat imperfectly, owing to the want of local faunas for most countries. With regard to the other orders, we are completely destitute of data for the countries out of Europe, except for a portion of the Lepidoptera. The following table, therefore, relates only to the Coleoptera. An attempt is made to compare with each other only countries of as nearly equal extent as possible; and it must be remarked, in particular, that the portion of Brazil, to which the notice is limited, scarcely equals France in extent.

Countries.	Latitude.	Authorities.	No. of Species.
Melville Island (Winter Harbour),	75° N.	Kirby,	0
Greenland,	60 70 N.	O. Fabricius,	11
Lapland,	64 71 N.	Zetterstedt,	813
Sweden,	56 69 N.	Gyllenhal, Paykull,	2,083
England,	50 61 N.	Stephens,	2,263
France,	41 51 N.	De Jean, and others,	4,200
Brazil, from Rio Janeiro to Bahia,	13 23 S.	De Jean, Klug, Perty, &c.	7,500

Notwithstanding the imperfections of this table, it sufficiently proves the proposition stated above. With regard to the other orders not included in it, every thing leads us to believe that they will shew a similar result, and that their progression from the poles to the equator will be, in regard to some of them, even more decided. Thus the whole of Europe and Siberia possess not more than 260 diurnal Lepidoptera, while the explored parts of Brazil, which do not nearly equal them in extent, have already furnished upwards of 600. The same country is an inexhaustible mine for Hymenoptera and Hemiptera; but our temperate regions, perhaps, present a less striking inferiority in Orthoptera, Neuroptera, and Diptera.

Genera are so vaguely determined in the present state of entomology, that a less satisfactory calculation can be made with regard to them. The number of them in a given country is not without importance, for it is they rather than the species which give to a country its proper entomological physiognomy. The following table is drawn up from De Jean's *Catalogue*, a work in which the generic groups are extremely numerous.

Country.	Species.	Genera.	No. of Species, per Genus.
Siberia, ...	465	169	2.7
Europe, ...	5,677	715	7.9
North America, ...	2,403	541	4.4
South America, ...	8,112	1200	6.7
Africa, ...	2,942	674	4.3
New Holland, ...	320	162	2.

From this it follows, that the absolute number of genera augments from the north to the south, since Europe has more than Siberia, and South America more than Europe; but it will be seen, at the same time, that this number does not increase in the same proportion as the species, but that it follows, on the contrary, an inverse progression. In reality, all the families, with a very small number of exceptions, have representatives in all the great regions of the globe; each of them will, consequently, include a smaller number of insects in proportion to the entomological poverty of these regions. The genera constituting these families have, in their turn, for the most part, their representatives or analogues in the same regions, and the rule of division, of which we now speak, necessarily applies to them likewise. From this it follows, that their number increases in an inverse ratio to that of the species; or, in other words, the number of genera in a given country will be relatively greater the fewer species that country possesses, and reciprocally.

Families being all represented in the same regions, except the Xylophagi and Pselaphidæ, they cannot be made the subjects of similar calculations. We might descend, it is true, to the groups immediately inferior, that is to say, to tribes; but it is well known how few entomologists are agreed on this point.

If, laying aside calculations, the precision of which is liable to suspicion, we desire to class the different regions of the globe according to their entomological riches, without attempting an unattainable exactitude, it will require to be done in the following manner.

In the first rank will be placed intertropical America; and at the head of the countries which it contains, Brazil; after which will come Mexico, then Guiana, and Colombia.

The isles of Sunda, the portion of the Indian Continent lying in their vicinity, Madagascar, Cafraria, and the eastern coasts of intertropical Africa, will occupy the second rank; but it is impossible in the present state of our knowledge to determine to which of these the preference ought to be assigned.

In the third grade we may place Europe, including the sides of the bason of the Mediterranean. Germany, taking that word in its widest acceptation, appears the richest country of this division.

North America seems to be much less prolific than Europe in the same latitude, and considering it as a whole, it appears to be, in the view we are now taking, on a level with Asia, which, although situate in part beneath the tropics, includes too many extensive sterile tracts to be so productive of insects as its geographical position would lead us to expect.

The same reason renders it necessary to place in the fifth rank Northern Africa, Chili, Tucuman, Peru, and in general the countries of America lying to the west of Brazil and south of Colombia.

The lowest in this comparative scale, must necessarily be the Polar Regions of the two Continents, to which New Holland does not seem much superior, notwithstanding its partially intertropical position.

The proportion in which the species, genera, and families, are found in different countries is susceptible of being established in a more satisfactory manner. It may even be shewn in a given country from a collection moderately ample, provided the entomologist who formed it has not collected certain families with greater care than others. This is unfortunately not the case, particularly with exotic species, for every collector almost always has an involuntary preference for certain groups. Yet we now possess sufficient materials for the coleoptera to enable us to attempt the task in question with regard to them.

The following table presents the number of species of this order as known at present in the eight following regions, not differing much from each other in extent, viz., North America, excluding Mexico, which, by its partially tropical position, belongs to the following region; South America and Mexico; Africa; Europe, along with Southern Russia; Lapland and Siberia; Asia; Indian Archipelago; finally New Holland. These regions appear sufficient for the purpose now in view. The species are grouped in twenty-two families, as in De Jean's *Catalogue;* but instead of disposing of them in systematic order, they are placed, for each region, according to the decreasing number of the species, an arrangement which enables us to see at one glance which of them predominate in such region. The table is principally designed to shew this predominance; and a few subsequent remarks will point out the other inferences, not unimportant, which may be deduced from it.*

The proportion between the families in each region is rendered manifest by this table, but that is not sufficient; it is

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* The basis of this table, which must have cost its author a great degree of labour, is the 3d edition of De Jean's catalogue; but all other accessible works descriptive of new species have been consulted. On combining his materials, he found that he had obtained cognizance of 24,650 species of Coleoptera, that is 2251 more than the catalogue contains.

North America.		South America, (including Mexico).		Africa.		Europe and Southern Russia.		Lapland and Siberia.		Asia.		Indian Archipelago.		New Holland.	
Families.	Species.	Families.	Species.	Families.	Species.	Families.	Species.	Families.	Species.	Families.	Species.	Families.	Species.	Families.	Spe.
Carabidæ	389	Chrysomelidæ	1853	Lamellicornes	685	Curculionidæ	1145	Carabidæ	182	Carabidæ	194	Curculionidæ	238	Lamellicor.	70
Chrysomelidæ	375	Curculionidæ	1615	Carabidæ	475	Carabidæ	1090	Brachelytres	135	Lamellicornes	172	Chrysomelidæ	192	Curculioni.	65
Curculionidæ	285	Longicornes	947	Chrysomelidæ	387	Brachelytres	683	Chrysomelidæ	121	Chrysomelidæ	135	Lamellicornes	116	Chrysome.	60
Sternoxes	211	Lamellicornes	905	Curculionidæ	314	Chrysomelidæ	610	Curculionidæ	117	Curculionidæ	130	Longicornes	105	Longicor.	35
Longicornes	186	Carabidæ	591	Melasomes	312	Lamellicornes	388	Clavicornes	82	Melasomes	112	Carabidæ	94	Sternoxes	32
Lamellicornes	172	Sternoxes	451	Sternoxes	213	Clavicornes	380	Hydrocanthares	72	Sternoxes	73	Trimeres	78	Carabidæ	30
Malacodermes	130	Malacodermes	445	Longicornes	190	Sternoxes	327	Longicornes	58	Longicornes	58	Sternoxes	72	Helopiens	15
Brachelytres	129	Clavicornes	192	Vesicants	132	Longicornes	303	Sternoxes	55	Vesicants	50	Malacodermes	51	Malacoder.	10
Clavicornes	117	Malasomes	185	Malacodermes	103	Melasomes	290	Lamellicornes	53	Clavicornes	31	Clavicornes	32	Melasomes	10
Trachelides	88	Helopiens	180	Clavicornes	75	Malacodermes	270	Xylophages	47	Trimeres	22	Taxicornes	30	Trachelides	7
Xylophages	85	Trimeres	147	Xylophages	56	Xylophages	215	Trimeres	38	Malacodermes	20	Tenebrionites	23	Hydrocantha.	6
Hydrocanthares	76	Xylophages	140	Trachelides	50	Trimeres	192	Malacodermes	37	Helopiens	18	Helopiens	18	Clavicornes	6
Teredyles	61	Tenebrionites	132	Trimeres	45	Hydrocanthares	156	Melasomes	27	Hydrocanthares	17	Trachelides	13	Tenebrioni.	6
Trimeres	47	Taxicornes	128	Helopiens	43	Teredyles	127	Taxicornes	25	Teredyles	14	Hydrocanthares	10	Stenelytres	5
Tenebrionites	40	Brachelytres	115	Hydrocanthares	38	Trachelides	114	Vesicants	22	Palpicornes	11	Brachelytres	10	Trimeres	5
Vesicants	35	Vesicants	88	Taxicornes	34	Vesicants	105	Palpicornes	20	Tenebrionites	10	Melasomes	7	Teredyles	4
Taxicornes	30	Trachelides	72	Tenebrionites	30	Taxicornes	88	Trachelides	16	Brachelytres	9	Teredyles	6	Brachelyt.	3
Palpicornes	27	Hydrocanthares	65	Teredyles	27	Helopiens	80	Teredyles	11	Taxicornes	8	Xylophages	5	Palpicornes	3
Helopiens	25	Teredyles	60	Brachelytres	22	Palpicornes	78	Stenelytres	6	Trachelides	8	Vesicants	5	Taxicornes	3
Stenelytres,	12	Palpicornes	51	Stenelytres	13	Pselaphidæ	63	Tenebrionites	5	Xylophages	7	Palpicornes	2	Vesicants	2
Melasomes	7	Stenelytres	20	Palpicornes	12	Stenelytres	57	Helopiens	3	Stenelytres	5	Stenelytres	2	Xylophages	0
Pselaphidæ	4	Pselaphidæ	5	Pselaphidæ	0	Tenebrionites	52	Pselaphidæ	0	Pselaphidæ	0	Pselaphidæ	0	Pselaphidæ	0
	2531		8387		3229		6813		1137		1104		1109		375

much more essential to know their geographical distribution according to their functions, that is, according as they derive their nourishment from vegetable or animal substances. Of this the table enables us to form a pretty accurate calculation.

The families essentially and solely creophagous are the Carabidæ and Hydrocanthares. Four others, the Brachelytres, Teredyles, Clavicornes, and Palpicornes contain a mixture of carnivorous and phytophagous species, probably about the one-half being of the former description, the other of the latter. All the other families are phytophagous.

Founding our estimate on these considerations, we find that of the 24,650 species mentioned in the preceding table, 4752 are creophagous, and 19,898 phytophagous, that is to say, the first are to the second : : 1 : 3,97.

A very different result will be obtained, if a similar calculation be made regarding the entire class of insects, for, according to the above estimate, the carnivora would be to the phytophaga about : : 4 : 9. Kirby and Spence making a similar inquiry regarding 8000 English insects (including Arachnides), estimate the proportion at nearly one-half. What renders the calculation more complicated when extended to the entire class, is, that a multitude of species are creophagous in their first condition, and phytophagous in their last, while in the case of the Colepotera the mode of nutrition is more uniform.

A second consequence resulting from the table, one already alluded to, is, that the relative proportion of the creophagous Coleoptera diminishes rapidly as we approach the equator. They bear the following relation to the totality of the species in the regions indicated :

New Continent.

North America, ...	: : 1 : 4,01
South America, ...	: : 1 : 9,59

Old Continent.

Siberia, ...	: : 1 : 2,90
Europe, ...	: : 1 : 3,87
Africa, ...	: : 1 : 5,55
Oceanea, ...	: : 1 : 8,59

But the creophagous species do not always take their food in the same state any more than the phytophagous. Some attack animals or plants while yet alive, while others find nourishment in dead animals and vegetables, in a more or less advanced stage of decomposition; or to employ Mr Macleay's phraseology, some are thalerophagous, others saprophagous. It is not without interest to inquire into their relative proportions, and the laws to which they are subject, for the parts which they perform in the economy of nature are very different.

Among the creophagous kinds, the Carabidæ, Hydrocanthares, and carnivorous Palpicornes, may be regarded as thalerophagous. There remain, consequently, only the Teredyles, the Brachelytres, and Clavicornes, which live on animal substances, to be ranked among the saprophaga. Of the 4752 creophagous species mentioned above, we find that 3587 are thalerophagous, and 1165 saprophagous. The latter consequently are to the former :: 1 : 3,07.

On the Natural History of Man. By M. Flourens.

The study of man, viewed in the light of natural history, has a peculiar importance, which no other branch of this science can pretend to. The physical characters which distinguish the *human races* from one another, are, perhaps, the *fact of natural history*, which has at all eras most struck the imagination of men. We know the great astonishment of the first Portuguese, who, in the fifteenth century, penetrating into the interior of Africa, found there men absolutely black, with curled hair, flat noses, and thick lips. This astonishment was renewed at the epoch of the discovery of the New World. Historians relate, that, on the first return of Columbus, the Europeans could not withdraw their eyes from the unknown plants and animals he had brought back, *and above all from the Indians, so different from all the races of men they had hitherto seen.*

Notwithstanding this lively interest which the physical study of man inspires, and has at all times inspired, this study is very little advanced. And first, with regard to the ancients, one can scarcely collect any thing else concerning the natural history of man, properly so called, in Herodotus, Strabo, even Galen, &c., than certain erroneous opinions about the nature and causes of the colour of the Negroes. The true founder of this new science is Buffon. His treatise on the *Varieties of the Human Species* is the first important step made in this department of study. But, for want of sufficiently sure *anatomical characters*, Buffon did not arrive at the precise determination of these *varieties.* He admitted a transition from the *negro* to the *white;* he believed that the *heat of the climate* was the only cause of the black colour; and he came to this conclusion, that all the physical differences which at present distinguish the varieties of the human species have only been, *originally*, the effect of *external and accidental causes.*

Camper is the first who sought *precise anatomical characters.* His observations on the profile of the *Negro* compared with that of the *white*, were a real advance; and Blumenbach, the venerable father of present naturalists, made a farther step, by extending to the entire conformation of the cranium and of the face this study of precise characters, which Camper had only applied to the *facial line.*

On the other hand, Malpighi, Albinus, Meckel, Cruikshank, Gautier, &c., endeavouring to determine the seat of the colour of the Negroes, opened a way which has been much more successful, although its full results have only been obtained in our own days, as we shall now shew.

Malpighi supposed that the layer of the skin, which he called the *corpus* or *rete mucosum*, was the seat of the colour of the blacks. Albinus and Meckel believed they demonstrated it; but from new anatomical researches, to which I have lately subjected the whole structure of the skin, it is evident that Malpighi, Albinus, Meckel, &c., had very confused ideas concerning the nature of the *corpus mucosum.*

In the *first* place, they supposed it arranged as a *network;* it forms, however, a continuous lamina. In the *second* place, they supposed it to be chiefly in the skin; it exists, in reality, only in the *mucous membranes. Lastly*, they supposed that this *corpus mucosum*, white in the white race, black in the black race, determined, by its colour alone, the colour of the men of these two races; it does no such thing.

There are in the skin of the *white* race three distinct laminæ or membranes—the *derm*, and two *epiderms;* and in the skin of the *black* race there is, besides the *derm* and the two *epiderms* of the *white* race, a particular apparatus, an apparatus which is altogether wanting in the man of the *white* race, an apparatus composed of two layers, the external of which is the seat of the *pigmentum* or colouring matter of the Negroes.

There is, then, in the skin of the *black* race, an apparatus which is wanting in that of the *white* race. The two races, the *white* and the *black* therefore, form two essentially and *specifically* distinct races. And these two races are not only distinct by a *character of form*, as the characters drawn from the conformation of the cranium and face are; they are so by a *character of structure*, by a special and very complicated apparatus, by an apparatus which exists in one of the two races, and is wanting in the other.

Buffon supposes that the black colour is only the effect of climate. He supposes, that *originally* the *Negro* was white. All these suppositions fall before the better known anatomy of the skin. The effect of climate can neither give nor take away an apparatus or tissue.

The individual, indeed, of the white race, may assume that swarthy dark complexion which is the effect of hot weather; but anatomy informs us, that the seat of this *swarthy complexion* is the *second epiderm*, and not a peculiar and distinct apparatus. On the other hand, the *Mulatto* results from the crossing of the *black* and *white* races; and the *colouring apparatus* of the Negro is always found in the *Mulatto.*

The *white* race and the *black* race are then, I repeat, two essentially distinct races. The same is true of the *red* or *American* race. Anatomy discovers, under the second epiderm of the individual of the *red, copper-coloured, Indian, or American* race (for this race is called indifferently by all these names), a *pigmental apparatus* which is the seat of the *red* or *copper colour* of this race, as the *pigmental apparatus* of the Negro is the seat of his black colour.

M. Cuvier says of the American race, "that, although it has not yet been clearly reduced to any of the races of the Old Continent, it does not possess *at once a precise and constant* *character*, which can make a particular race of it;" and he adds, that the *copper-red complexion is not a sufficient one.* He would assuredly have been of a quite contrary opinion, if he had known that the *copper-red tint* is owing to a special determinate apparatus, an apparatus which anatomy detaches and isolates from all the other parts of the skin.

In considering, I do not say *the characters of form*, but the *characters, the differences of structure*, there are then three specifically and primarily distinct races—the white or *Caucasian* race, the Negro or *Ethiopian* race, and the red or *American* race.

Such are the results which I have explained in my lectures of late years in the Museum. It is true that, for want of favourable opportunities, I have not yet been able to extend these researches of structure to the other races, and particularly to that which, among all others, appears most important, I mean the *yellow* or *Mongolian*. I have hitherto been reduced to characters of the second order, *characters of form*, viz. characters taken from the conformation of the cranium and of the face.

I call these last characters of the *second order*, and by this are explained the differences which exist among naturalists regarding the determination of the *human races*, a determination which, in truth, has been hitherto founded only on these characters. M. Blumenbach fixes the number of these races at five—the *Caucasian*, the *Mongolian*, the *Negro*, the *American*, and the *Malay*. M. Cuvier reduces these five races of Blumenbach to three—the white or *Caucasian*, the yellow or *Mongolian*, and the Negro or *Ethiopian*, and nevertheless he confesses, that "neither the *Malays* nor the *Americans* can be clearly referred to one or other of these three races." Lastly, a more recent author, the learned M. Pritchard, raises, and always by regulating himself according to the form of the crania, the number of the *human races* to seven. The first four are, the *Caucasian*, the *Mongolian*, the *Negro*, and the *American* (except the *Esquimaux*, who form a separate tribe); the fifth is that of the *Hottentots* and *Boschismans;* the sixth that of the *Papuans*, or the people with curled hair of *Polynesia;* and the seventh that of the *Alfourous* and *Australians*, As for ourselves, by adhering to the authentic crania alone which our Museum possesses, we think we can establish ten distinct forms or types of human heads—the *Caucasian* type, the *Mongolian*, the *Negro*, the *American*, the *Malay* or *Javanese*, the *Hottentot*, the *Boschisman*, the *Papuan*, the *Alfourou*, and the *Zealandic*.

I shall recapitulate, in a few words, the principal characters of these types.

The *Caucasian* type is distinguished by the oval head, the height of the cranium, the prominence of the forehead, that of the nose, &c.; the *Mongolian* type by the lateral prominence of the cheek bones, the square form of the cranium, &c.; the *Negro* type by a compressed forehead, a flat nose, oblique incisor teeth, &c.; the *American* type by the volume of the posterior part of the cranium, the prominence of the nose, the width of the orbits, &c. &c.

M. Pritchard has suppressed, as we have just seen, the *Malay* type. This type wants indeed, even in Blumenbach who established it, precise characters. I think I have found these characters on two heads in our Museum, one of a *Javanese*, the other of a *Madurais;* two heads whose type is quite similar, and both of which are distinguished by the projection which the very large parietal protuberances make behind, and above all by the manner in which the occipital bone is abruptly flattened below these protuberances.

The cranium of the *Hottentots* evidently forms a particular type, beside the general type of the *Negroes;* this cranium is long and narrow, but it is proportionally very elevated; and by this also it is distinguished, in a marked manner, from the cranium of the Boschismans, which is, on the contrary, singularly flattened, and as it were crushed from above downward.

The *Papuans*, carefully described by MM. Quoy and Gaimard, and the *Alfourous*, described with no less care by M. Lesson, are two distinct types. The *Papuans* are remarkable for the flatness and the depression of the forehead and the face; the *Alfourous* have a long and narrow cranium. I add, that, if the sinking which the parietal bones on each side of the sagittal suture present, on the two heads from Van

Diemen's Land, in our museum, was found constant, it would be sufficient to indicate a variety in the type of the *Papuans.*

Finally, the last of the types which I propose, the *Zealandic*, is marked by the height and narrowness of the cranium, especially in front, by the extent of the temporal fossa, by the anterior prominence of the apophysis of the chin, &c.

All these types are only founded on secondary characters; and, consequently, they have not the importance of the three *primitive races*, founded, as we have seen, on the characters of structure. It follows also, from the circumstance of the characters which constitute them being only secondary, that many of these types ought to enter as *sub-races*, either into one of the three *primitive races* already established, or into some other of those *races* which may hereafter be established.

However this may be, I have made use, in my lectures, of these types, provisionally admitted to refer to fixed and determinate groups, the observations which have been collected concerning different people by travelling naturalists, such as Forster, Bougainville, Peron, &c., and more recently, Lesson, Quoy, Gaimard, Garnot, &c.

Besides, to these characters, drawn from the cranium and the face, all those other characters, the union of which constitutes their force, are to be added:—the colour of the hair, the prominence of the lips, the opening of the eyes, &c., and even the habits of those savage tribes which may be regarded as primitive, and consequently as a more immediate effect of their organization itself. I do not speak here of the characters drawn from the languages, characters of a very elevated order, but the development of which we must derive from another science.

Our business here is to establish *anatomical characters.* I have proposed, therefore, in my lectures, three principal objects:—the first, to seek *anatomical characters* which distinguish the human races from each other; the second, to follow the modifications which these *characters* experience in the filiations of these races, from the *primitive race* to the *sub-race*, and from the *sub-race* to the *tribes* and *families* derived from it; and the third, to go back to the particular laws which preside over the distribution of the particular branches of the human species on the different points of the surface of the globe.

The study of these three parts of the physical history of man, formed the subject of my lectures of late years. But I cannot finish this article, without examining a question which is at this day much controverted;—I mean, if the *different races* of men form a single *species*, or if, forming different species, they constitute what is called in natural history a *genus*. A simple glance at the definition of the word *species* will be sufficient to cause all difficulty on this point to vanish.

Buffon defines species,—"A succession of similar individuals which reproduce each other." M. Cuvier also defines species,—"The union of individuals descended from each other or from common parents, and of those who resemble them as much as they resemble each other." Now, it is easy to see that this definition, given by Buffon and M. Cuvier, is complex, and that it unites two very distinct facts, viz. the fact of *reproduction* or of *succession*, and the fact of *resemblance.*

Here the fact of the *resemblance* is completely subordinate to that of the succession; and Buffon and Cuvier afterwards agree in this themselves. "The comparison of the *resemblance* of individuals is only," says Buffon, "an *accessory idea*, and often independent of the first (the idea of constant *succession* by generation)." "The apparent differences of the races of our domestic species," says M. Cuvier, "are stronger than those of any savage species of the same genus."

Besides, the appreciation of *resemblance* is always more or less arbitrary. One naturalist often finds a *resemblance* important which another naturalist considers slight. The foundation of all natural history (for the foundation of all the natural history of organized beings is the positive determination of the species) would only repose, then, on an arbitrary appreciation.

The idea of *resemblance*, as Buffon says, is only an *accessory idea.* It is, in other words, a *subsidiary means* which naturalists employ for want of the only *decisive means*, the fact of the *succession;* but when the *decisive means*, the fact of the *succession* is known, the *subsidiary means* ought to be excluded.

The fact of the *succession*, therefore, and of the *constant succession*, constitutes alone the *unity of the species*. Thus, *unity*, *absolute unity*, of the human species, and *variety* of its races, as a final result, is the general and certain conclusion of all the facts acquired concerning the *natural history of man*.*

On certain Circumstances affecting the Colour of Blood during Coagulation.† By PATRICK S. K. NEWBIGGING, M.D. F.R.C.S.E., formerly Senior President of the Royal Medical Society, one of the Medical Officers of the New Town Dispensary, &c. (Communicated by the Author.)

During the summer of 1834, whilst engaged in prosecuting some inquiries into the state of the blood in eruptive diseases, my attention was called to the appearance of various vermilion-coloured spots, represented upon that portion of the coagulum which had been in contact with the lowest part of the cup, in which the blood drawn from a patient had been received. On examining the interior of the cup, I found that the outline of that portion of the ornament which was of a green colour, corresponded exactly with the figure of the vermilion-coloured spots on the clot; upon the rest of which, where in contact with any of the other colours with which the interior of the cup was decorated, no change was observable. The circumstance appearing curious, and not as yet, as far as I knew, noticed, I stated the fact in a short communication to the Royal Medical Society. Thereafter I was induced to repeat the experiment with the same cups in several instances in which I had occasion to draw blood, and always with a similar result; the green invariably striking a bright red, contrasting strongly with the dark hue of the surrounding coagulum. With the view of satisfying myself that the appearances were altogether unconnected with any diseased condition of the blood, I procured an individual in perfect health, from whom I let blood in the ordinary way. In this, as in other instances, the green produced precisely the same vermilion figures. Finding this to be the case, I determined to ascertain, as far as I had an opportunity of doing so, whether, and to what extent, any other colours produced a similar effect; for as yet I had seen no change from contact with any colour excepting green. My interest was still further increased, by understanding from Dr Abercrombie that he had made a similar remark. With this view I procured a variety of cups, with devices of various colours decorating their interior; and these I used in many cases in which I had occasion to draw blood, but without succeeding in obtaining any change of colour upon the coagulum, except from the green. This was invariable, provided the colour was decided and brilliant; the impression on the clot being always an accurate representation of the ornament on the cup. The result of these observations I communicated in a short notice I read to the British Association at their meeting in Dublin in August 1835.

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* Annales des Sciences, Nat. t. x., Dec. 1838, pp. 361, &c.

† Read before the Royal Society of Edinburgh, March 1839, by Professor Forbes, who, along with Sir John Robison, witnessed several of the experiments.

Owing to an absence abroad, it was not till lately that I had an opportunity of resuming my inquiries into the subject. During the last two or three months, however, I have made a considerable number of experiments, under various circumstances, with the view of ascertaining some of the conditions which affect the result.

I procured a considerable number of cups, with devices of different descriptions both as regards figure and colour, and subjected them to the usual method of making such observations, as often as a suitable case presented itself. In order also to trace if possible any connection, if such existed, betwixt the substances of which the ornament was composed, and the appearance on the blood, I obtained, through the obliging introduction of Mr Child of this city, a communication from Mr Ridgway of Staffordshire, giving much practical information as to the different ingredients which are used in ornamenting china. From him I learnt, that the bright green colour is composed of the green oxide of chrome, and laid on as a pigment in the usual way. Whilst another and very common method of ornamenting china was, by laying upon the biscuit previous to its being glazed, a device printed and coloured upon paper. By this mode there is no perceptible elevation of the ornament, and the colours are not so deep or brilliant.

In the progress of numerous investigations, I found that there was invariably an accurate representation of the ornament, provided the colour was of bright green, and slightly elevated as it is when affixed to the china by the former operation, but could observe no effect whatever in those cases where the decorations appeared to have been produced by the latter process. With the view of further illustrating the subject, I caused the seven prismatic colours to be painted on separate lozenges at the bottom of a cup, and had them coated with copal varnish. Out of four experiments which I made with this cup, I procured no change excepting in one instance, and that was merely a light mottling, barely appreciable, but in no respect corresponding with the effect produced by the green ornament in my former experiment. Up to this period, there appeared to me to exist two conditions, either of which might effect the result, namely, the brightness of the green ornament. and its partial elevation on the surface of the china; —the first being apparently connected with some chemical cause; and the latter possibly with the pressure upon the clot, or the order of distribution of the globules of which it is composed, as affected by the elevation of the ornament.

Oxygen being the recognised agent, producing the change from the dark purple of the venous, to the bright vermilion tint of the arterial blood, in the absence of any other probable explanation, it suggested itself to my mind that, possibly, there might be some peculiar affinity betwixt the bright green and that element of the atmospheric air, causing some of it to linger on the surface of the ornament, when the rest of the air had been expelled from the cup by the entrance of the blood. I resolved therefore to try whether another substance, similarly affected by oxygen, would produce any thing like a similar result. For this purpose I made a solution of isinglass with protoxide of iron, the effect of oxygen on which is to convert its leaden colour into that of a reddish brick, and poured it liquid into a cup, with a bright green device, that had, in every instance in which it had been employed, struck the corresponding vermilion image on the blood. On inverting the coagulum which was thus formed, after the upper surface, exposed to the air, had assumed the red colour, it was found that the rest of it, in contact with the interior of the cup, presented the leaden colour, without any trace whatever of change of tint on the part corresponding with the green ornaments. Unsuccessful in this, in order to assist me to estimate how far the other condition which I had observed, namely, the partial elevation of the green ornament, was connected with the result, I had artificial elevations made in the interior of the cup, by fixing a small piece of wood of the size of a button mould in the bottom of it, covered with copal varnish. During repeated experiments, the only effect from this was a depression corresponding to the elevation, but no perceptible change of colour. Still further to test how the effect might be produced by the impression of the elevation on a coloured surface, I substituted isinglass coloured with cochineal, which, when coagulated, and inverted, somewhat resembles the clot of blood. In this instance there was a depression corresponding to the elevation of the green ornament, but no change of tint.

Satisfied that the appearance could not be accounted for by any mere mechanical effect produced by the impression of the elevation, it occurred to me that possibly the blood in coagulating might so arrange its particles in relation to the ornament, as to produce the phenomenon described. With a view to test this, I drew blood into a plain white cup, and, after complete coagulation, removed the clot into one whose decoration had on many occasions given the vermilion colour. After allowing it to remain here for some hours, I again inverted it in the usual way, and procured a most accurate and beautiful representation of the ornament as in my first observations. This of course entirely contradicted this hypothesis.

Thus far in my observations I had remarked nothing to disturb my conviction of the appearance being connected with the colour of green. In no instance, and I had made upwards of sixty observations since I first noticed the circumstance, had I been able to observe the slightest difference of tint, when the ornament was of another colour. Upon a recent occasion, however, when employing a small cup with the device of a flower, of a pinkish or crimson colour, darker towards the circumference of the petals, and shaded off towards the centre, I was much surprised, on inverting the clot, to find a most perfect representation of the flower upon its surface, not only giving its outline with the utmost accuracy, but the different shades of the bright red corresponding in intensity with the darker and lighter parts of the ornament, so as to present to the eye a very fair image of the flower itself. On examining the cup minutely, it appeared to me that the colours had been laid on by the brush, and the margin of the petals of the flower being darker than towards their interior, was proportionally elevated. Subsequent experiments with the same cup produced the same result. This is the only instance, however, in which I have observed an effect from any other colour than green; and, during the course of my observations, I have employed cups with almost every variety of ornament, both as regards device and colour.

It is unnecessary to add, that this single observation at once put an end to any conjectures which I might have formed as to the green colour being indispensably connected with the effect. Whilst, on the other hand, it has left me still more in the dark than ever, as to the cause of the appearance I have ventured to describe. Nevertheless, I have thought it right to detail, in the way I have done, the different steps I have pursued in my inquiries, unsuccessful though these have been, in the hope that my doing so may aid in suggesting to others, some explanation of the phenomenon which I have been unable to give, as well as facilitate the inquiries of those who may be disposed to prosecute the investigation farther. The subject is one which has interested me not a little; and I shall certainly not lose sight of any opportunity which may offer of procuring additional information that may tend to throw any light upon it.

On the Form of the Globules of the Blood in some Mammifera.[*]

The blood fulfils, in the animal economy, so important a part, and its history is connected with so many questions full of interest to science, that the study of it must naturally fix the attention of a great number of physiologists; and when the discovery of the microscope came to enlarge the field of their investigations, they did not fail to inquire if this powerful instrument could not reveal to them some new character in the nourishing fluid of animals. This hope has not been disappointed, and, by the aid of the microscope, one can easily convince himself that the blood, far from being formed only of a liquid holding different substances in solution, is essentially composed of innumerable solid corpuscles, which swim suspended in a particular fluid, and which affect constant forms. Malpighi appears to be the first who remarked the existence of these corpuscles; he did not, however, form an exact idea of their nature, and it was principally to a man of a much less elevated genius, Leeuwenhoek, that the merit of the demonstration, if not the discovery, of this truth belongs. His first observations go back to 1673, and this date is also that of our first precise notions concerning the form and nature of the globules of the blood.

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[*] From "Rapport fait à l'Academie des Sciences le 31 Decembre 1838,

Jurin, Senac, Muys, Fontana, Hewson, afterwards added new facts to those established by Leeuwenhoek, and rectified some errors into which this observer had fallen. The researches of Hewson, above all, deserve to be mentioned with praise; and from this series of labours has resulted a body of valuable physiological knowledge. But towards the end of last century, the microscope had the fate of many other new inventions. After having its utility exaggerated, and having been employed to support the foolish speculations of genius, men fell into the opposite extreme; they magnified its inconveniences and dangers; then they almost entirely neglected its employment, and only spoke with distrust of the greater number of the results obtained by its aid. They even went the length of calling in question the existence of the globules of the blood, and ascribed to optical illusions what Leeuwenhoek and his successors had said concerning them. For a

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sur une note de M. Mandl, relatif à la form des globules du sang chez quelques Mammifères, par M. Milne Edwards." Ann. des Sc. Nat. t. xi. Jan. 1839. Pp. 46, &c.

time, then, the discoveries of micrographers were, in some measure, lost to physiology; and it was necessary, in order to reintroduce them into science, that they should obtain the sanction of modern observers, whose labours bore the seal of those rigorous minds, which will not allow themselves to be convinced until they have acquired all the proofs necessary to convince others. This re-establishment of the microscope in the eyes of physiologists does not go twenty years back, and it is not one of the least services which MM. Prevost and Dumas have rendered to science by the publication of their researches on the blood.

Among the curious results obtained by these two observers, there is one which, formerly perceived by Hewson, ought especially to interest zoologists,—the coincidence of a certain *form* in the globules of the blood, and of certain peculiarities in the general plan of the organization of the animals in which they have studied them. In different individuals of the same species, the corpuscles which give to the blood its colour, are all, with very few exceptions, similar to one another, both in relation to their dimensions and their form. In animals of different species, their dimensions may vary, and these varieties are sometimes very great, even in beings which, in other respects, have a striking resemblance; but the *form* of the globules of the blood only appeared to change from one class to another, and not to vary in the different animals belonging to the same natural division of the animal kingdom. In all the mammifera, indeed, submitted to their examination, MM. Prevost and Dumas have constantly found that these corpuscles were circular, and resembled small discs marked with a central spot equally circular; while in birds, reptiles, and fishes, they have always seen these globules elliptical, and provided with a spot in the centre, of the same form, which appeared to them to be an internal nucleus.

About the same epoch Rudolphi announced that in the blood of many fishes, such as the perch, the plaice, and the sole, the globules were circular, similar to those of the mammifera; but more accurate observations have shewn that this physiologist was led into error by the alterations which these corpuscles easily undergo under the influence of water and many other agents.

This exception, then, to the general rule, deduced from the observations of MM. Prevost and Dumas, does not exist in reality, and new microscopical researches on the physical constitution of the blood, made by a great number of physiologists in Germany, England, and France, have successively enlarged the bases on which it rested. MM. Prevost and Dumas had, it is true, established the existence of circular globules in the embryo of the chick during the early period of incubation; but, in animals which have already passed the period of the metamorphoses characteristic of the embryo state, no similar anomaly was known, and after a considerable number of particular observations already collected, it appeared to be a legitimate conclusion that, in vertebrated animals, the blood with circular globules belongs essentially to the mammifera, and that the blood with elliptical globules was peculiar to the aves, reptilia, and pisces. Now, these two groups of vertebrated animals differ also from each other in their mode of reproduction, and it was not uninteresting to observe that the blood, in all oviparous vertebrated animals, differed by so clear characters from that of the vertebrated provided with mammæ.

M. Wagner, in a recent publication, has announced that, in the lamprey, the globules of the blood are circular; but the lamprey is a fish so abnormal, and appears in so many relations to approach to invertebrated animals, in which the solid corpuscles suspended in the nourishing fluid are also circular; that this exception seemed to be explained by the nature even of the animal in which it was found, and did not appear to be sufficient to diminish the importance attached to the differences of forms, already observed in superior animals, between the globules of the blood of the mammifera and oviparous vertebrata.

Such was the state of this point of science when M. Mandl presented to the Academy the note of which we are going to give an account, and if we have entered into these historical details, *a little too minutely perhaps*, it is because they appeared to us necessary in order to make us duly appreciate the interest of the new observations submitted to our examination.

By pursuing researches on the microscopical characters of different organic tissues, researches which he proposes to collect into one work, some parts of which are already before the public, M. Mandl has been led to examine the blood in different animals. The menagerie of the Royal Garden, always open to those who seriously desire to profit by its riches in order to advance science, has afforded him an opportunity of greatly multiplying his observations on this subject, and of arriving at very unexpected results. He at first established that in a great number of mammifera whose blood had not yet been examined by the microscope, for example, the *papion*, a *guenon*, a *sajou*, the *coati*, the *kinkajou*, the *elephant*, the *tapir*, the *hemionus*, and the *stag*, the globules are circular as in all the other mammifera already studied with regard to this particular; but he found afterwards that *in the dromedary it is quite otherwise. There, the globules of the blood, instead of being circular, are elliptical as in birds, reptiles, and fishes.*

The dromedary belongs, as we know, to a small natural family, in the order of ruminants, and is represented in the Old World by the genus *camel*, and in the New Continent by the genus *lama*. It became consequently very interesting to see if the singular anomaly offered by the blood of the dromedary would be found also in the blood of the lamas. In order to resolve this question, M. Mandl has taken advantage of an *alpaca* in the menagerie of the museum, and, in the note addressed to the Academy, he announces the fact that in the blood of this animal the globules are also of an elliptical form.

Your commissioners have, along with M. Mandl, repeated these two observations, and are satisfied of their accuracy. In *dromedaries* of both sexes, as well as in the *alpaca*, the globules of the blood are certainly elliptical: their great diameter is about $\frac{1}{135}$th of a millimetre, and their small diameter about $\frac{1}{250}$th. These corpuscles are, as you see, smaller than those of any known bird, reptile, or fish, and approach in their dimensions the globules of the other mammifera. The elliptical central spot, which they present, appears also to result from a depression rather than from the presence of a prominent nucleus; finally, it is also proper to remark that the blood of these animals, as well as that of the other mammifera, contains, besides these red globules, certain white and rounded corpuscles of more considerable volume, corpuscles which M. Mandl believes to be formed of fibrine. We shall add farther, that, in order to preserve ourselves as much as possible from the causes of error to which observations of this sort are exposed, we have examined by the microscope the drop of blood immediately after extracting it, and to retard its coagulation, after extending it in a very thin layer, we have only covered it with a plate of glass, without adding any thing to it. Lastly, our observations have been made with a microscope of Chevallier, magnifying about 500 times, and our measures have been taken by the aid of a clear chamber adapted to this instrument.

We wish we had been able to examine in like manner the blood of the camel with two protuberances, and that of the vigogne, in order to assure ourselves if this character, so abnormal in the class of the mammifera, is found in all the species of the family of the camelidæ; but the menagerie does not possess any at present.

In oxen, sheep, goats, antelopes, and stags, the globules of the blood are circular; is it the same in the girafe, which, in certain respects, approaches more to the camels? This question has appeared to us to merit examination, and to resolve it, your commissioners, in concert with the author of the researches, of which we are now giving an account, have submitted to microscopical examination a small drop of the blood of the girafe in the Museum, obtained by means of a slight puncture made in the lip of this animal. But the globules did not present any thing particular to us; they are circular as in the ordinary mammifera, and are in diameter about $\frac{1}{160}$th of a millimetre.

Thinking that the blood of the *marsupialia* might, like that of the camelidæ, present some anomaly, we have examined it also in the *kangaroo with whiskers*. But here, too, the globules are circular, and it is only worth while to remark, that their magnitude is less uniform than in most of the mammifera, and that their dimensions have appeared to us to vary between $\frac{1}{145}$th and $\frac{1}{125}$th of a millimetre.

These new facts appear to us to increase the interest of the observations made by M. Mandl; for they shew how general the tendency of nature is to give to the globules of the blood of the mammifera a circular form, and consequently they also add to the singularity of the exception established by this young micrographer, an exception which shews how much caution is necessary when, in comparative physiology, one draws general conclusions from even a very considerable number of particular facts. The study of organised bodies reveals to us the tendencies of nature, but has conducted us very rarely to the knowledge of the laws which govern her works.

It were much to be wished that naturalists would not allow any opportunity to escape, in order to complete our knowledge concerning the form, the dimensions, and the structure of the globules of the blood; for an exception to a rule, in appearance as well established as that relative to the constancy of this form in each of the classes of vertebrated animals, may make us suppose that there are other similar anomalies; and it is perhaps by means of these exceptions that we will ultimately be able to seize the relations which ought, very probably, to exist between the physical characters of these corpuscles, and other peculiarities of the organization. This is a subject of research which we would recommend to travelling physiologists, and to those whose position in large menageries allows them to multiply and vary their observations; for experiments of this nature do not subject the animals submitted to them to any danger, and may give results full of interest to physiology. It would appear to us, above all, important to examine, in this respect, the blood of the Monotremata, Edentata, the Phocæ, and the Cetacea, among the mammifera; that of the Crocodiles, the Sirens, and the Axolotls, among the reptiles; and among the fishes, that of the Bonites, whose temperature, according to Dr J. Davy, approaches that of warm-blooded animals. If, in this list, we omit the Cassowary and the Ostrich, which, of all the birds, are the most abnormal, it is because your commissioners are already assured that, with relation to the form and dimensions of the globules of the blood, these animals differ in no respect from all those of the same class already observed by micrographers. These corpuscles are really elliptical, and have appeared to us to have, in the *Cassowary of New Holland*, about $\frac{1}{79}$th by $\frac{1}{133}$d of a millimetre, while in the *Naudou*, they are perhaps a little less elongated; for the mean of the measures we have taken has given $\frac{1}{73}$th by $\frac{1}{133}$d of a millimetre.

With regard to the note of M. Mandl, it may be seen that we have found his observations exact and interesting, and we accordingly propose to the Academy to view them favourably, and to engage this physiologist to prosecute the researches, an account of which we have now given.

Some Account of Violent Columnar Whirlwinds, which appear to have resulted from the action of large Circular Fires; with Remarks on the same.[*] By Mr W. C. Redfield.

Since my attention was first attracted by the phenomena of our great whirlwind storms, I have found frequent occasions for noticing the points of analogy, and also of dissimilarity, between these great storms and the smaller classes of whirlwinds which are known under the various names of squalls, thunder-gusts, tornadoes, water-spouts, sand-pillars, and the like. While pursuing this inquiry, I received information of a few cases in which whirlwinds of great activity and violence, appeared to have resulted from the action of fires. The facts attending these cases, as then related to me, were carefully noted, and were laid aside with a view, at a future period, to incorporate them with a more general account and examination of the smaller description of whirlwinds, than I have yet found it convenient to undertake. In the mean time, the verbal statement of these cases, both in Europe and this country, through the medium of a friend who is distinguished for his attainments in science, appears to have exited some interest in the minds of meteorologists and others, and has occasioned applications to be made to me for a full statement of the facts. I therefore hasten to give them publicity, without the delay which the execution of my original purpose would necessarily occasion.

The most recent of these cases, being the first of which I obtained notice, occurred in the year 1830, in the township of

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[*] Read before the Conn. Acad. of Arts and Sc., Jan. 22. 1839; afterwards published in vol. xxxvi. of Silliman's Journal.

Greenbush, near Albany. My account of it was obtained soon after, from William Akin of Greenbush, an observant and highly respectable member of the Society of Friends, on whose farm the phenomenon happened. The facts were carefully taken down in the presence of Mr Akin, from his statements, and the account thus obtained was, by his assistance, carefully revised and corrected.

Statement of William Akin.

"In the year 1830, I had cut the timber from a small tract of woodland comprising about twenty-five acres, and the brushwood throughout the field had been piled and prepared for burning. Previous to firing it, the brushwood lying near the outskirts of the field was moved inwards, in order to prevent the spreading of the fire to the surrounding woodlands, the materials thus removed forming a circular range or heap around the general mass of combustibles in the area of the field. On a warm and perfectly calm day in the summer, this circle was fired on all sides, nearly at the same time. The smoke and flame soon gathered towards the centre of the field, where they whirled and ascended with great rapidity, in a single column. With the strength of this whirl the fire rapidly increased, and the heat and flame from opposite sides, pressing inwards towards the ascending column, the latter continued its spiral or whirling motion with great rapidity and violence. It was a magnificent spectacle; and was attended with a loud, roaring noise, and a sort of crackling and nearly continuous thundering; resembling that which I have heard in a violent hail-storm.

This remarkable noise, which I think might have been heard at a distance of several miles, was also accompanied by frequent and loud snappings or explosions, resembling the reports of muskets and pistols, as sometimes heard in an irregular running fire of militia.

This roaring noise and the powerful whirling motion of the column, continued for a period of about twenty minutes, as near as I can now estimate. The swift whirling of the whole, exceeded all my previous conceptions of the velocity of wind. The height of the smoky column seemed almost as great as the eye could reach. At times, the column would assume a sinuous or wavy direction, and would again become straightened into its upright position."

I learned also from Mr Akin, that he has on several occasions noticed a whirling motion in the clouds during the exhibition of a violent hail-storm, and that such storms, according to his observation, are always attended by a continuous thundering roar, not unlike that of the above-described whirlwind. One of these hail-storms he described as passing within three roods of the house in which he resided; tearing up trees, scattering their limbs in the air, and desolating a path or track of about fifty yards in width. Another of these storms, of a more extended character, Mr A. describes as very destructive to the farm on which he then lived, and as having left the ground covered with hailstones, to the depth of four or five inches, and in particular situations, to the depth of even twenty inches. This hail-storm acted on a tract of near two miles in its entire width, and was also attended with the above-mentioned peculiar noise in the atmosphere during its continuance, and also with heavy thunder and vivid lightning. In this last case, no whirlwind was felt at the surface, but he supposes one of considerable magnitude to have been in action in the atmosphere.

For an account of another interesting case of the same character, and which I received not long after the above, I am indebted to the Hon. Theodore Dwight, now of Hartford, Conn. and formerly a resident of this city. It was related to me more than once by this gentleman, for my gratification, and was, at my request, drawn up by himself, and kindly placed at my disposal.

Statement of Theodore Dwight, Esq.

"In the month of April 1783, I resided at Stockbridge, in the state of Massachusetts. The season, for a number of weeks previous, had been remarkably dry, and in various places much mischief had been done by fires which were kindled in the woods. Beside other fires, one occurred upon a mountain lying between Great Barrington and Stockbridge, which spread and advanced till it reached the northern termination of the mountain, south of Stockbridge, near the river Housatonic, which runs through the town. Near the foot of the mountain at its northern termination, was an open field, in which a large quantity of bushes and brushwood, that had been cut some time previously, in clearing the field, was lying in rows and heaps for burning, and had become perfectly dry and combustible. The owner of the field, as the fire reached near its border, sent some men to set all on fire around the field, in order to consume the whole together. The weather was mild and serene, and the atmosphere perfectly still and undisturbed. I was residing at this time at the distance of about half a mile from the fire, and my attention was suddenly excited by a loud, roaring noise like that of heavy thunder; whereupon, going to the door, I instantly discovered the cause. Upon the fire becoming general throughout the field, a whirlwind had formed in the midst of the flames, and when I first saw the phenomenon, its appearance was sublime and awful. The flames were collected from every side into a large column, broad at the bottom, but suddenly tapering to a much smaller size, and it stood erect in the field to the probable height of 150 to 200 feet. It was a pillar of living and most vivid flame, whirling round with most astonishing velocity, while from its top proceeded a spire of black smoke, to a height beyond the reach of the eye, and whirling with the same velocity with the column of flame. The noise produced by this whirlwind, was louder than almost any thunder I have ever heard; and being much longer continued, was heard at a greater distance than is commonly the case with thunder. During the whole period of its continuance, the pillar of fire moved slowly and majestically round the field; but generally the air was entirely free from both fire and smoke, except what was collected in the column. The spire of smoke above the pillar of fire, not only whirled around with the most surprising rapidity, but owing to its great height, waved gracefully in the air, which added much to the beauty and splendour of the exhibition. The force of the whirlwind was so great, that young trees of six or eight inches in diameter, which had been cut and were lying on the ground, were taken up by it, and carried to the height of forty or fifty feet.

The scene was to me perfectly novel; and though it occurred nearly fifty years ago, is still clear and distinct in my recollection, and it was one of the most magnificent spectacles that I have ever witnessed.

"*New York*, 28*th November* 1831."

Mr Dwight related, also, that the men who lighted up the field, became so alarmed by this whirlwind of fire, as to rush for safety to the neighbouring river.

During a visit to Amherst, in Massachusetts, in the month of August 1832, I obtained from Dr Cowles, of that place, the following account of a similar case, which occurred in that vicinity, under that gentleman's immediate observation.

Statement of Dr Cowles.

"In the summer of 1824, I had prepared, for burning, the refuse timber and brushwood, on seven acres of pine woodland, which had been cut over for some months previous. A still day, which proved very warm, was selected, in order to avoid the danger of extending the fire to the neighbouring woodlands by means of the wind; and as the materials were in fine combustible order, I invited several friends to witness the burning. The combustibles were collected in piles and ridges, and fire was set to the outward portions of the field, on all sides, as fast as was conveniently practicable. The smoke and flames were now concentrated in a large whirling column, over the centre of the field, rising in the form of a cone, and ascending to a great height. It was attended with a heavy roar, which was heard at a great distance. Although on a perfectly calm day, the action of this whirlwind was so violent as to remove from the ground large pieces of brushwood, even from places not touched by the fire, carrying them high in the air, from whence they afterwards fell without the limits of the burning field."

I learned also from Dr Cutler, of Amherst, that his attention had likewise been drawn to this phenomenon, which he beheld from near his own house, at a distance of a mile and a half from the fire. He informed me, that it exhibited to his view, an elevated pillar of black smoke, attended by a "roaring noise, like that occasioned by the violent burning of a chimney."

These statements were received from the above gentlemen, and separately and carefully noted down on the spot.

An intelligent farmer, a resident of Delaware county, N. Y., whom I met on the 9th of May 1832, informed me that he had on several occasions seen whirlwinds formed in burning over newly cleared lands ; and had known them so violent as to take up heavy limbs or branches into the air. He had recently seen one in that country, which moved up the side of a hill, on a still day, and prostrated trees in its course.

In the burning of a wooden building, I have myself seen a momentary effect which seemed analogous to the foregoing; and a temporary impulse of this sort, I believe, is not uncommon in large fires.* I have, however, seen no distinct account of phenomena like the foregoing, in those great fires which attend the burning of cities or forests, even if during a gale ; if I except the account contained in the following extract, which appears to indicate something of this kind on an extended scale,

" *A Great fire.*—Miramichi is mentioned† as connected with one of those tremendous fires which sometimes arise in the American forests, and spread havoc by circles of longitude and latitude. In the autumn of 1825, such a calamity occurred on the river Miramichi, which extended 140 miles in length, and in some places 70 in breadth. It is of little consequence that no wind was stirring at the time; for, as Mr M'Gregor observes, the mere rarefaction of the air creates a wind, " which increases till it blows a hurricane." In the present case, the woods had been on fire for some days without creating any great alarm. But " on the 7th of October, it came on to blow furiously from the westward; and the inhabitants along the banks of the river were suddenly surprised by an extraordinary roaring in the woods, resembling the crashing and detonation of loud and incessant thunder, while at the same instant the atmosphere became thickly darkened with smoke. They had scarcely time to ascertain the cause of this awful phenomenon, before all the surrounding woods appeared in one vast blaze, the flames ascending from one to two hundred feet above the top of the loftiest trees; and the fire, rolling forward with inconceivable celerity, presented the terribly sublime appearance of an impetuous flaming ocean. Two towns, those of Douglass and New Castle, were in a blaze within the hour; and many of the inhabitants were unable to escape. Multitudes of men on lumbering parties perished in the forest; cattle were destroyed by wholesale; even birds, unless those of very strong wing, seldom escaped, so rapid was the progress of the flames. Nay, the very rivers were so much affected by the burning masses projected into their waters, that in many cases large quantities of salmon and other fish were scattered upon their shores. Perhaps the plague of fire has never been exhibited, or will be, till the final destruction of this planet, on so magnificent a scale."

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* In the month of August last (1838), a similar phenomenon was observed in the burning of one of the large wood-yards and wood-houses, belonging to Yale College. The yard was a rectangular area, with the wood arranged chiefly on the outer boundaries, and the same ascending column of flame and smoke was distinctly observed by many, but more particularly by Mr B. L. Hamlen, the printer of this Journal.

† Miramichi is in the British province of New Brunswick, near the southwestern borders of the Gulf of St Lawrence.

I am unable to give the authorship of this paragraph, which came to my hand through the columns of a newspaper; but it appears to have formed part of a more extended notice of that region of country; and the occurrence of this calamitous fire, is a fact well remembered.

Remarks on the foregoing Cases.

Among the many considerations suggested by these interesting phenomena, I shall notice in a cursory manner, the following:

1. *The rarefaction which is ordinarily produced by fires, even if they are of an extensive and violent character, is not attended with these effects.* Common observation and experience are sufficient to determine this point, and to shew that rarefaction *alone*, is entirely inadequate to the production of such results. The strongest degree of heat which has ever been created in the atmosphere by fires, has never produced an ascending current of sufficient power to prostrate or carry forward any contiguous body possessed of tolerable stability or weight, much less to carry any such body into the air.

2. *The foregoing results can be accounted for, only by a violent vortical action, steadily maintained.* This appears too plain to need illustration.

3. The origin of the rotative action and its continuance for a considerable period, in the circuit of conflagration, *appear to be chiefly owing to the circular outline of the several fires, and to the absence of a disturbing horizontal current.* When, however, the principal seat of the vortical action is found in the body of an elevated current of atmosphere, we then find that the progressive action of the foot of the whirling column upon the earth's surface, and the resistance which is also offered to its progress by an inferior cross current, are not sufficient to break up, or sensibly impede, the regular action and progress of a powerful vortex. This was strikingly manifested in the tornado which passed through the city of New Brunswick, in New Jersey, in June 1835.

4. *The ascending power of the vortical column or whirlwind is, in these cases, strongly exhibited.* We observe, that the heated air from large fires, or even from the most powerful furnaces, ejected in a column at the heat of melted iron, will ascend only to a comparatively short distance from the earth; the ascending force being lost in the counter-movements and convolutions with the adjacent colder air, and the combined product soon spreads off in a horizontal direction. But the spire of a columnar vortex, exhibits a penetrating and ascending power which far exceeds, both in its intensity and the extent of its action, any other ascending movement that we witness in the atmosphere. This effect appears to be owing to the spiral motion of the column, which presses onward in the direction of its axis, until it reaches a limit of elevation which is yet unknown. Even the *ring vortex* which is sometimes seen to form at the muzzles of cannon or the ejection pipes of high-pressure steam-engines, on their discharge, appears to possess the qualities of a projectile, notwithstanding its unfavourable form; and is also carried forward through the air, partly on the rocket principle, by means of the rotary action by which the circular axis of the ring is involved; the line of progress in this case being in the direction which is perpendicular to the

plane of the ring.* On the proximate causes and *modus operandi* of this ascending action in the columnar vortex, we cannot now dwell.

5. *The analogy of the foregoing cases to those violent columnar whirlwinds which are so often formed over the craters of active volcanoes, and the apparent identity of the causes which produce them,* well deserve our notice. We may hence comprehend the manner in which volcanic ashes, having no projectile qualities, are carried to a vast height in the atmosphere, and become wafted to a great distance by the different currents of the atmosphere into which they successively subside. The loud roaring noise and thundering detonations, which usually attend these volcanic exhibitions, are also illustrated to some small extent, by the cases before us.

6. *It appears that these cases were attended by electric explosions or detonations,* and that, apparently, there was only wanting the contact of more extensive masses of the higher and lower atmosphere, and the presence of a larger body of aqueous condensation, as in the case of the so-called *thunder cloud* which so commonly attends the naturally formed vortex, in order to have produced the phenomena of genuine thunder and lightning, on a most magnificent scale. This effect, it appears, is most fully produced in the volcanic exhibitions above mentioned.

7. These cases may serve to illustrate, in some degree, the favouring influences which are presented in the *calm latitudes*, near the equator, and on the exterior limits of the trade winds in mid-ocean, for the production of vorticular action, either in the form of squalls, thunder-gusts, or water-spouts. The main horizontal movements of circulation which are common to our atmosphere, being here comparatively sluggish or inert, opportunity is thus afforded for the minor influences of rarefication to come into play; by which means, frequent squalls, water-spouts, and other local movements in the lower atmosphere, are excited in these regions.

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* The vortex here described is also produced in a beautiful manner, by the burning of bubbles of phosphuretted hydrogen gas, as they escape from water. In this case the peculiar movements of the ring vortex, as well as the sustaining and expanding power which appears to belong to aërial vortices, may be advantageously observed.

8. The velocity and strength of the wind in the rotative action of a vortex, the axis of which is in a vertical position, as in the above cases, greatly exceeds that which is exhibited in other circumstances.

9. We may perceive, also, in these cases, how effectually the heated stratum of air nearest the earth's surface, may be caused to penetrate a homogeneous and colder overlying stratum of current, by means of the vorticular action; a movement which is otherwise physically impossible, except in a partial degree, by means of other concurrent movements or favouring circumstances.

10. These considerations, and others which suggest themselves, will enable us to account for the sudden and rapid formation of hail, in summer hail-storms and tornadoes; and in the structure of the hailstones, in successive layers, and their occasionally fractured condition, we may perceive both the evidence and the effects of the violent vortical action, and the successive changes of temperature and hygrometric condition in which they have been successively placed, before leaving the vortex, and while falling to the earth.

11. These cases, viewed in connection with the natural phenomena which have been exhibited in a multitude of instances, appear fully to confirm the opinion of Franklin, in favour of the general identity of the columnar whirlwinds with the so-called *water-spouts.*

12. By the clue which is here afforded us, in the peculiar noise and rumbling detonations of these violent whirlwinds, we may become apprised of the existence of a violent vortical action in the atmosphere, or within the envelopment of a visible thunder-cloud, even in those numerous cases in which the vortex or whirlwind does not reach the earth's surface.

13. Perhaps we have also, in these noises, a clue to the sounds which are ascribed to certain moving sands, in the heat of summer, as in the case of *Jebel Narkous*, or the sounding mountain, near Tor, on the Red Sea,* and also in the *Reg-ruwan,* on a hillside, near Cabul, which is described by the Emperor Baber and by Capt. Burnes. From the descriptions

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* Wellsted's Travels in Arabia, Lond. 1838. Vol. ii. pp 23–27.

given of the localities and the other circumstances which attend these sounds, which, however, cannot be here recited, I have been led to infer that they proceed from the action of a whirlwind formed on the leeward side of the hill, and revolving upon a horizontal axis; analogous in a degree, as I suppose, to the celebrated Helm Wind of the Cross Fell Mountain in Cumberland (England).

The inquiry opened to us by the consideration of these phenomena is extensive and interesting, in all its bearings, and I cannot but regret my inability to pursue it with the attention which it so well merits.

New York, December 31. 1838.

On the Reproduction of the Virgularia or Pennatula mirabilis. By Sir JOHN GRAHAM DALYELL. Communicated by the Author.

Few animal products are so little known as this, which is one of the most singular and beautiful in the universe; nor, in as far as I am aware, is any correct representation of it extant. None that I have seen bears even a distant resemblance to it, in the living state, excepting that of the indefatigable Müller, which seems, nevertheless, as much the result of recollection, as of actual delineation at the moment of inspection.

The original is not to be recognised from preserved specimens.

Having previously explained the various modes observed by me, wherein different genera of zoophytes perpetuate their race, I propose to devote a few words here to that of the subject in question.

The virgularia preserved in spirits bears some imperfect resemblance to a feather, whence the name *pennatula*, recently converted to *virgularia*. But, although difficult to be described, let the reader rather conceive a yellow fleshy rod, environed by a numerous series of oblique thin leaves, forming three-fourths of a circle, with eight or ten small marginal knobs on each, unfolding into as many asteroidal hydræ. The remaining fourth of the circle is quite smooth and uniform. A central bone, surprisingly slender for its length, occupies the middle of the rod.

When inactive, the pennatula is of a flattened figure, not exceeding three lines across. When active, or in a state of intumescence, it would occupy three-fourths of a cylinder nine or ten lines wide—the remaining fourth being nearly vacant, from the flattened side above described. But, in respect to length, we are at this day altogether ignorant of its real dimensions; for it is a product never to be obtained entire. Commonly, we find it in fragments seven or eight inches long.

The largest specimen occurring to me extended twenty-three inches, of which the bone occupied no less than eighteen. But this also was mutilated, for an inch of the bone at one end was bare, and had been so for some time, as the habitation of another animal was seated on it. From the portion of the flesh lost here, and from that extending beyond the opposite extremity, the specimen, entire and vigorous in its native element, could not have been shorter than thirty inches. The bone is probably the longest cylindrical osseous or shelly product known, its length equalling about five hundred times its diameter.

The Virgularia mirabilis, and that zoophyte to which I formerly alluded as the Sertularia *uber* (or *argentea*, according to the recent learned and useful work of Dr Johnston), both dwelling in the Scottish seas, perhaps exceed the length of any others hitherto distinctly specified.

The multitude of animals and of organic parts composing them surpass belief. On a specimen of the virgularia seven inches long I computed 130 leaves; on each leaf 20 hydræ, each with 8 tentacula, and 20 pinnæ on each tentaculum; thus affording 416,000 organic parts belonging to the specimen, all subject to volition. But it is probable, that, in the larger specimen above described, the parts under control exceeded a million ! ! !

In common with most inhabitants of the deep, the virgularia is strictly a nocturnal product, whose action commences with the approach of night; for it is then only that its intumescence, relaxed throughout the day, and the vigour of its numerous hydræ, are manifested.

Whether it enjoys a locomotive faculty, or whether, growing upright, it is rooted to the same spot, has not been yet ascertained. If lying flat at the bottom of the sea, its progression must be extremely slow. Kept in a tall jar, the body forms itself into several volutes. For example, a specimen, eight inches long, will exhibit a single spiral throughout the whole length at one time, and at another, there may be three or four spirals—or, as we should say, *threads of a screw*—occupying its extent.

On the whole its nature bears an intimate resemblance to the Pennatula phosphorea, and to that of the Lobularia, more conveniently recognised as the *Alcyonium digitatum*, but, unlike the latter, it exhales an agreeable and somewhat musky odour. The hydræ of all three appear to correspond in conformation.

The Virgularia mirabilis inhabits a small circumscribed spot said to have a muddy bottom, immediately west of Inchkeith in the Frith of Forth.* Having employed two fishermen to dredge there early in June, I procured a number of specimens, but none exceeding eight inches in length. The more vigorous being selected, they were deposited in suitable glass vessels, and kept in a temperate atmosphere. Frequent inspection under such circumstances is essential, in order to ascertain the uninterrupted purity of the liquid element, for the slightest oversight here will render all experiments abortive.

In three days, yellow corpuscules, somewhat ovoidal, were observed both ascending the side of one vessel as well as free in the water, and also descending. Some rose ten inches high amidst it, with a swift and active motion. Removing several by means of a watch-glass, I subjected them to the microscope, where they proved of a smooth, consistent, fleshy, uniform aspect, of an elongated ovoidal form, without any visible external organs, and rapidly traversing the element. In all respects they narrowly resemble the planulæ of the *Sertulariæ*, nor did they leave me any doubt of their animal nature.

On the 25th of June, that is twelve days after the preceding animals had appeared in the water, three nascent pennatulæ were discovered on the bottom of the vessel, in a horizontal position, with a hydra or polypus developed from one extremity of each, identically resembling those of mature specimens, and inclining upwards.

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* Many years ago, we dredged up in Prestonpans Bay, Firth of Forth, fine specimens of Virgularia mirabilis.—Edit.

Afterwards, I succeeded in obtaining seven, which were brought to a situation sufficiently favourable for microscopical observation.

Here they remained, as also in other vessels, surviving until the end of July; but in none was more than a single hydra developed, which, towards the close, ceased to display itself, though the individual still lived, as is seen in other zoophytes.

But from the first they never shifted their position. This was always horizontal, and by a very slight adhesion: therefore, I am rather induced to believe that if they are erect in their native site, as some fishermen assert, adhesion results from a loose fleshy portion, extending below, beyond the bone. I must acknowledge, however, that I am incapable of arriving at any satisfactory conclusion on this subject.

Thus, much in conformity with the propagation of most zoophytes, a yellow ovum issues from the adult pennatula, which, as in some others, is seen in the embryonic state, as I conceive it, of a planula endowed with great activity. Its motion ceases, the planula becomes more elongated, rather cylindrical, and next a hydra with eight tentacula is developed from one extremity. At first the pinnæ forming the pectinate part of the tentaculum are few; stronger towards its extremity, and decreasing in size until becoming mere projections as they descend. The stomach is seen in the centre with four organs like cœca below. But I have not been able to recognise the central bone.

Some indications of a circulating fluid, by the ascent and descent of minute dark particles, as if contained in an invisible vessel, reaching as high as each side of the stomach, merit further investigation. Microscopical illusions are ever ready to mislead observers.

The planulæ are very minute, not above a third of a line in length, at their utmost dimensions; and the nascent pennatulæ, though extending considerably as they lay, never exceeded two lines if attaining as much. They were always of a fine uniform yellow colour, the body very flexible and of variable shape; and they invested themselves with such muddy particles as came in contact with them.

Remarks on Glaciers. By M. Agassiz.[*]

In laying before you, gentlemen, a few details regarding glaciers, and the phenomena attending them, I design to confine myself exclusively to matter of fact. I shall submit to you the principal observations which have been made on this interesting subject; to these I shall add a few others which I believe to be new; and finally, bring to your recollection some which have been known from the time of Saussure, but probably forgotten, because he did not connect them with the phenomena of glaciers, on which they really depend.

A glacier is a mass of ice hanging on the sides of an Alpine ridge, or enclosed in one of its valleys, and which is moving *continually* down the declivity. I say continually, for the glacier is always descending; if the extremity should at any time seem to retire, this implies nothing more than that the portion of the ice, melted by the heat of summer, is more considerable than that which the glacier brings along with it in its progress.

This movement of the ice, which many refused for a long while to admit, is now known and acknowledged by every observer; but there is a great contrariety of opinion respecting the cause which produces it. The opinion generally received from the time of Saussure, is, that the descent of a glacier is nothing more than a slipping upon itself, occasioned by its own weight. But there are many reasons for doubting the accuracy of this explanation. The motion appears to be much more properly ascribed to the expansion of the ice resulting from the congelation of the water which has filtered into it and penetrated its cavities. The ice of glaciers, it must be observed, has not the continuous texture of ordinary ice; it is composed of a multitude of fragments, which have been improperly called crystals by Hugi. We may easily convince ourselves of this by breaking a portion, or infusing a coloured liquid which penetrates into the fissures separating the fragments, and allows us to distinguish their form and size. It is easy to perceive that their size diminishes in proportion as we ascend either from the bottom of the glacier towards the surface, or from its lower to its upper part or origin. Here they may be seen reduced to mere granules, so that the ice, losing more and more its transparency and compactness, insensibly passes (nearly at a uniform elevation among the Alps) into the state of a coarse snow which is known to the mountaineers by the term *firn* or *haut névé*. A glacier is, therefore, a spongy mass, continually imbibing atmospheric waters, as well as those produced by the melting of its surface, and which infiltrate into the capillary fissures which the ice presents throughout its whole thickness, and particularly at the portion nearest the surface where it is less compact. The temperature of this water being always near the freezing point, it is converted into ice by the least sinking of the temperature, and tends to dilate the glacier in every direction. But as it is restrained on two sides by the flanks of the valley, and above by the weight of the superior masses, the whole act of dilatation, aided besides by that of gravitation, tends to urge it down the declivity to the only side which offers a free passage. This explanation being once admitted, it follows that the more frequently the alternations of freezing and melting take place, or the variations of the temperature are above and near zero, the more rapid will be the advance of the glacier subjected to them. Thus it happens that winter, when the entire mass is frozen in an equal manner, is the season when it is in a state of rest.

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[*] Read to a meeting of the Geological Society of France.

The progress of the glacier is not uniform throughout the whole thickness of the mass; but if we suppose it divided into beds parallel to its surface, each of these beds or layers will advance with greater rapidity in proportion as it is nearer the surface, or, in other words, as it is more exposed to the influence of atmospheric changes. It will be perceived that this difference in quickness will become more obvious in the upper beds, because there must be added to the quickness, proper to each of them, that of all the beds inferior to it; so that if the bed at the bottom move with the quickness of 1, the second with the quickness of 2, the third of 3, and so on, the quickness of the third, for example, will be 3+2+1 or 6.

A glacier, when seen in a vertical section, often exhibits a series of beds of variable thickness, sufficiently distinct in the upper part, less evident in the middle, and more or less obliterated below, according as the mass, from being exposed to moisture, has been more or less completely converted into transparent ice. These beds diminish in thickness from the top downwards, no doubt by an effect of the *tassement*, and represent the additional beds which the glacier receives every year. (Upper glacier of the Grindelwald, Trient, &c.)

In regard to external form, a glacier usually presents a more or less convex surface, particularly at the lower extremity. This form results from the reflection of the heat from the sides of the valley, which accelerates the melting of the ice at the edges of the glacier. When the ground on which the glacier moves, is but little inclined and free from inequalities, the surface continues regular, and the mass is not divided. But if it has some obstacle to surmount in its progress, or if the ground present one of those sudden changes of level so frequent among the Alps, the mass splits transversely into irregular leaves, moving on their lower edge as around an axis, and separated by wide crevices, which close again when the ground becomes less steep, just as the waves of a torrent again become calm after a fall. A glacier, in fact, is a river of ice stereotyped, with its cascades, rapids, storms, and calms; the superficial mass moving more quickly, and the lateral portions being influenced by the form of the bed in which it moves.

The destructive action of atmospheric agents on the mountain summits from which glaciers descend, and on the crests and declivities which border the valleys in which they move, the fall of avalanches, and the motion of the ice itself, are continually detaching, along the whole basin of the glacier, fragments of rock of every size, which roll into the place which the glacier occupies, and rest upon its surface. These debris, thus deposited on and carried along with the glacier in its progress, give rise to several remarkable phenomena. The largest of these fragments, by protecting the part of the ice which they cover from the action of the sun's rays and from rain, and also from evaporation (which is often considerable, being occasioned by warm or dry winds), become, by the sinking of the rest of the surface, gradually insulated on the summit of a large pedestal or pillar of ice. This support, suffering in its turn from the action of the same agents, soon gives way; the block rolls down, and forms another pyramid at some distance. It is these that are called the *tables des glaciers*, of which fine examples are afforded by the glaciers of the Aar. If these fragments do not exceed an inch in diameter, a phenomenon of a different description takes place. Absorbing the solar rays, by their property as opaque bodies, more rapidly than the ice, their entire mass (not the surface merely, as in large blocks) becomes raised to a high temperature. Instead, therefore, of protecting the ice beneath them, they cause it to melt, and form holes which often penetrate to a great depth; they even pierce the glacier from one side to the other; for as long as a constant cause of heat remains at the upper orifice, the water which fills them is warmed above zero, then descends by virtue of its maximum of density to the inferior beds, where it continues to perforate the ice by slowly melting it. When we add to these phenomena the small currents of water running in every direction, uniting into torrents, and throwing themselves in cascades into the larger crevices which open or close by turns, we shall be enabled to form an idea of the perpetual movement going forward of the surface of a glacier.

These blocks, scattered over the glacier, thus move along with it, and at last reaching its edges, and being continually thrown off, they accumulate and form masses of debris more or less considerable, which are named *moraines* among the Alps. These moraines are either *lateral*, disposed along the glacier parallel to its sides; or *terminal*, bounding its lower extremity, and usually describing a semicircle; or finally, *median*, forming long tracks on the surface of the interior of the glacier itself. These latter are occasioned by a union of the two lateral moraines of two glaciers descending two different gorges, and uniting in the same valley. The two glaciers never become blended, as might be supposed; each preserves its own direction and rate of progress, the line of separation being the two lateral moraines, which touch each other in such a manner as to form only one. However, when the progress of the two glaciers is very unequal, something like a division of the moraine takes place, and we then see two or three parallel tracks, as in the glacier of the Aar. These median moraines produce the phenomena of *tables des glaciers* on a large scale. Being placed at first in the depression formed by the union of the two convex surfaces of the contiguous glaciers, and protecting the ice which they cover from evaporation, they are soon elevated on a base of ice, usually in the shape of an ass's back, which, however, disappears where the moraine spreads out towards its extremity. (Glacier of the Aar.)

Let us now examine what is the action of the ice on the surface which it traverses. Here, also, we find fragments of rocks, which, by being pressed and ground, as if between the stones of a mill, are comminuted, or arrive, in the form of rounded pebbles, at the lower part, where they form the base on which the extremity of the glacier, and also the terminal moraine itself, usually rest. While moving along a rocky alterable surface, the ice, by modifying it, produces various phenomena, the principal of which are the following:

It levels it by the friction, and polishes it sometimes as perfectly as could be done by the marble-cutter, cutting the fossil bodies and concretions which it meets in its progress, and exercising its action equally upon the bottom of the bed and its sides.

It rounds off all the angles and inequalities of the ground, giving them a mammiform appearance, or transforming them into protuberances with rounded surfaces. (Granites of the Grimsel, *lapiaz* of the Valais.)

When the ground admits, it scoops out broad furrows, from an inch to a foot in diameter, the length of which is in the direction of the movement of the ice, and of these the surface is equally polished, and the angles rubbed off. Here, also, might be mentioned spoon-shaped depressions, resembling the commencement of a furrow which has not been continued, occasioned by certain movements of the ice, for which it is difficult to account. They might be called the cuts of a chisel in the flat surface of the rock.

Particles of the hardest sand, which are always found between the ice and the rock, such as small crystals of quartz, &c., produce the same effect as so many diamonds, by forming lines on these polished surfaces, which are thus covered with a multitude of rectilinear striæ parallel to each other. These striæ have no dependence on the structure of the rock; they do not follow its cleavage; they are seen to cut in two the crystals they traverse; they are always in lines of great inclination, and follow the direction which the form of the subjacent ground has given to the ice, whether in its regular progress or accidental deviations. They cannot be attributed therefore, as has been done by Deluc, to rapid currents of water, nor to muddy currents filled with fragments of rocks, as some other observers are inclined to believe. The *débâcle* of the Dent du Midi, which presented a fine example of a current of this nature, has not left any trace of this kind in any part of its course.

Finally, we perceive, on surfaces which the ice has left, other furrows, not rectilinear but undulated, often running into each other, and generally following the line of the greatest declivity. These are called *Carrenfelder* in some parts of the Alps. These furrows are evidently owing to the erosion of waters circulating beneath the glace, and gradually scooping out a bed down the declivity. Other erosions are likewise observed, exactly resembling those produced by a cascade in the place where it falls, and which probably have no other origin.

All these actions of the ice are somewhat modified by the nature of the rock on which they are exercised. Granite becomes rounded in large masses, and broad convex surfaces of a pretty uniform description. The masses in limestone are smaller, and acquire the most perfect polish. It alone presents beautiful surfaces, resembling those of a slab of marble from the hands of the workman. Gneiss and the schists are more furrowed, although often in a direction transverse to their beds.

These phenomena, at once well known and easily observed, afford us the means of ascertaining the former existence of glaciers at a remote period, in places where they do not now occur. When a glacier advances, it rolls before it all the blocks accumulated at its extremity, and which form around it a more or less semicircular mound. But many glaciers, on parting from the moraine which borders their extremity, present several others, concentric to the first and of a like structure; of these, the most remote is necessarily the most ancient, and the intermediate ones mark so many epochs of the successive recedence of the glacier. It is thus that Saussure formerly detected moraines in the valley of Chamouni, although they were partly covered by vegetation. From the extremity of the glacier des Bois or of the Mer de Glace, we can, in fact, enumerate seven concentric moraines, as far as the village of Tines, which is built on the last and largest, at the height of more than 200 feet. Ascending towards the Col de Balme, ten or eleven others are distinguishable, although many causes are continually in operation to obliterate them. These collections of rolled stones undoubtedly indicate the ancient extent of the glaciers. We see, in the particular case alluded to, that the valley of Chamouni was occupied by an immense glacier, receiving in its course those of Argentière and of Tour, and moving towards the Col de Balme, in order, probably, to debouche in the Valais by the Col de la Tête-Noire.

Polished and rounded surfaces, with furrows and striæ still remaining, particularly on hard rocks, notwithstanding the destructive influence of the atmosphere, serve even now as guides to the site of ancient glaciers, in places where they would never have been supposed to exist. Saussure, whose eye nothing escaped, had long since remarked them on the extensive polished rock of St Bernard (see vol. ii. p. 451. 4to ed.), as well as in other places, but he failed in divining the causes of these appearances. They are seen to exist beneath the ice, within the moraines; they also appear on the outside on the ground, and on the declivities and sides of the valleys, accompanied, often to a great height, with old lateral moraines. It is thus that we can follow the striæ beneath the ice of the Aar almost without interruption as far as the Hospice de la Grimsel, around which all the granite is deeply striated. They likewise appear throughout the whole valley of Hasli, particularly on the beautiful rounded surfaces of the Helle-Platte, below Handeck. M. Braun, an engineer, has noticed them at Leissingen, on the left bank of the lake of Thun ; M. A. Guyot, at Oberwald, in the upper Valais. The whole of the lower Valais is, in like manner, the bed of a glacier, of which the lateral moraines may be distinguished at a pretty considerable height above the valley. I have taken care to indicate these on the map (that of Keller) as I have determined them from Martigny to Lausanne. Polished surfaces, striæ, and all the phenomena produced by the action of ice, appear more beautiful and distinct on the Jura range than in any other place, accompanied likewise with Alpine blocks disposed in two zones, one above the other, the second rising to a point upwards of 2000 feet above the level of the plain. The identity of these polished surfaces and striæ with those observed among the calcareous rocks of the Alps—for example, below the glacier of Rosenlaui in the Bernese Oberland,—the distribution of rolled blocks on the sides of this chain, and their relations to the Alpine moraines, leave no doubt as to the former existence of a layer of ice which covered all the great Swiss valley, and moved towards the north-east, in the direction of the declivity. This icy covering would be no doubt circumstanced somewhat differently from a glacier enclosed within a narrow Alpine valley, but the principal characters, notwithstanding, must have been the same.

From the whole of these phenomena, and the circumstance of analogous striæ having been observed in Sweden by M. Sefstroem, I conclude, that at a certain epoch the whole of Europe was covered with ice : that this epoch marks the date of the disappearance of the large mammifera which have been discovered among the ice of northern regions ; that it must have preceded the elevation of the Alps ; but that the recedence of the ice, the polished surfaces, the moraines, and the dispersion of the boulders to the very summits of high mountains, must all have been posterior to the elevation of the Alps to their present level.

Anatomical Researches on the Structure of the Gastric and Intestinal Mucous Membranes.[*] By M. FLOURENS.

"I have shewn, in a former memoir, that the mucous membrane of the lips, the mouth, and œsophagus, is composed of three distinct membranes,—the dermis, the mucous body, and the epidermis.

"Before me, none had studied the mucous body, except on the tongue: I have traced it on the lips, the cheeks, and in the œsophagus. Besides, it was supposed, according to Malpighi, that the mucous body of the tongue was arranged as a network: but I have shewn, on the contrary, that it forms a continuous membrane.[†]

"With regard to the epidermis, it was early observed on the lips, in the buccal cavity, and œsophagus; and some anatomists even went farther, and believed they recognised it in the stomach and the intestines. But the opinion of the existence of the epidermis in the stomach and intestines has never yet been generally admitted.

"Thus, at the end of the 17th century, Glisson maintained that the epidermis was wanting in the intestines, and that its place was supplied by the lubricating mucus.[‡] More than a hundred years after Glisson, Bichat denied anew, and in a manner almost as absolutely as Glisson, the existence of the epidermis of the stomach and intestines. "In the stomach," says he, "and the intestines, the most delicate instrument cannot separate the epidermis: in the maceration and boiling of these parts, I have never seen the epidermis raised on their surface. I have extracted from the belly of a dog a portion of intestine, and applied an epispastic to it, but no blister arose. From these considerations," continues he, "it appears that the epidermis does not exist on these mucous membranes. At least, it cannot be admitted before a new examination, which, I believe, will prove rather against, than for, its existence."[§]

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[*] Acad. des Sciences, 27th May 1839. Pp. 833, &c.

[†] Voyez *Comptes rendus*, années 1837–38.

[‡] Glisson *De Ventriculo et Intestinis.*

[§] Bichat, *Anat. Gener.* t. iv. p. 471.

"Beclard is of the same opinion as Bichat. 'This question,' says he (of the existence of the epidermis on the mucous membranes of the stomach and intestines), 'cannot be resolved otherwise than it has been by Bichat, who,' adds he, 'inclines rather to the negative.'[*]

"Lastly, Meckel, who, like Beclard, appears to have undertaken the task of subjecting to a new examination almost all the great labours of Bichat, expresses himself on this question in terms still more decided than Bichat himself. 'Epispastics,' says he, 'during life, and maceration after death, are unable to demonstrate the existence of an epidermis on the villous coat of the stomach and intestines. It is very doubtful, then, whether this epidermis exist, and whether, as Haller and Bichat think, its existence be attested by the formation of membranes having the form of the channels whence they escape, since the formation of these membranes can be very well explained otherwise.'[†]

"Thus Glisson, Bichat, Beclard, and Meckel, deny the epidermis of the stomach and intestines: but, on the other side, many anatomists, not less celebrated, admit it; as Ruysch, Lieberkuhn, Haller, &c.

"Ruysch admits it, and calls it *epithelium*. He is not sure, however, that he has really separated it from the villous coat of the intestines; for he says, *that one sees the villosities of this coat without requiring to take away the epithelium.*[‡]

"Lieberkuhn is, I believe, the first who has clearly seen the epidermis of the intestines, and perhaps the only one of these old anatomists who has seen it well. He says that a membrane, similar to the epidermis, covers the villosities of the intestines; and, what is still more precise, he says, that this membrane is continuous with the epidermis of the stomach, the œsophagus, and mouth.[§]

"Haller also admits the epidermis of the stomach and intestines; but what takes much weight from his assertion is, that he seems everywhere to confound the true epidermis with the villous coat of these parts.*

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[*] Beclard, *Notes sur Bichat.*

[†] Meckel, *Manuel d'Anat.* t. i. p. 499.

[‡] Ruysch, *Thesaur.* vii. No. 40.

[§] Lieberkuhn, *De fabr. et act. vill. intest.*

"Since Haller, many able anatomists, particularly M. Doelinger, have seen the epidermis of the stomach and intestines; and M. Doelinger has made the following very just remark:—That the villosities of the intestine are enveloped by this epidermis, as the fingers of the hand are by the fingers of a glove.†

"Of all the anatomists whom I have just cited, Lieberkuhn is the first who has fully recognised the epidermis of the gastric and intestinal mucous membranes. He is, farther, the first who, in order to detach this epidermis from the villous coat, has proceeded in a regular manner: that is, by a skilfully conducted maceration.

"It is also by the aid of a skilfully conducted maceration, and conducted, too, with precautions not observed by Lieberkuhn, that I have succeeded in detaching the epidermis of the stomach and intestines; and in detaching it, not by fragments and shreds, nor by a sort of good fortune and chance; but by entire and continuous laminæ, in a sure and steady manner.

"I have the honour of placing before the Academy, two portions of the small intestine, which present, in their whole extent, the epidermis of the villous coat, everywhere distinct and detached from this coat.

"This epidermis forms a fine, transparent, continuous membrane. The external surface is rough, with small projections; the internal surface presents a multitude of small depressions. These external projections and internal depressions, mark the points of the epidermis which correspond to the papillæ of the dermis, and which serve as sheaths to their papillæ.

"But it is not only an epidermis, a proper and continuous membrane, which is seen on the two pieces now placed before the Academy. You see also, and particularly on the piece No. 3, a true mucous body, interposed between the papillæ of the dermis and the epidermis, a little thicker than the epidermis, and forming the first sheath of the papillæ of the dermis, whose epidermis forms only the second.

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* Haller, *Elem. Phys.* t. vii. p. 22.

† Doëlinger *De vasis sanguiferis, &c.*

"The lamina of the mucous body resembles exactly that of the epidermis: quite rough, like it, with small projections on its external surface, and covered all over with small depressions on its internal surface.

"It is necessary, however, to add that, while the sheaths of this mucous body remain attached to the papillæ of the dermis, this body forms a true network; but a factitious network, a network which, like the famous *network of Malpighi*, or of the mucous body of the tongue, only depends on the artificial adherence of the sheaths of the mucous body to the papillæ of the dermis.

"I have found this same structure of a mucous membrane composed of three superimposed membranes, the dermis, the mucous body, and the epidermis, on the stomach; and that notwithstanding the extreme fineness of the mucous membrane of this organ.

"I may say that the papillæ, and particularly the papillæ of the small intestine, otherwise so remarkable, only appear in all their richness and admirable regularity when they are, as they are seen on the piece No. 1, which I present to the Academy, stripped both of the mucous body and the epidermis, which, in their ordinary state, mask and cover them.

"I may say farther, that it is only then that one is fully assured of all the generality of this fact, already established in my preceding memoirs, viz. that the villosities or papillæ are every where productions of the dermis, that they belong every where to this dermis, and that the mucous body and the epidermis only serve as sheaths or envelopes to them.

"A second fact, not less important, and which acquires new force from these new researches, is that the general character of mucous membranes, even those most deeply situated, is to present a dermis covered with a mucous body and an epidermis, just as the general character of the skin is to present a dermis covered with a double epidermis.

"Finally, it is easy to see that an improved knowledge of the structure of the stomach and intestines, gives a new aid to the physiological study of these organs. It has always been repugnant to physiology, and that, notwithstanding the authority of the very great observers Glisson, Bichat, Beclard, and

Meckel, to admit that this internal surface, this papillary surface of the stomach and intestines, the seat of the most delicate and active functions of the economy, and on which the most irritating and energetic substances act, is deprived of all other means of protection, except the simple mucus, more or less abundant, by which it is lubricated.

"Now, as we have just seen, the internal and papillary surface of the stomach and intestines is not, in reality, a naked surface. It is covered by two continuous and superimposed membranes, whereby it comes under the general law both of the skin and the mucous membranes, already considered in my preceding memoirs, that is, being subjected, as the skin and these membranes to the incessant action of external bodies, it is covered, like them, by two superimposed and protecting laminæ."

Account of the Parallel Roads of Glen Roy, in Invernesshire.[*]

Mr Darwin, in a memoir lately read before the Geological Society of London, *on the Parallel Roads of Glen Roy*, gives a rather novel view of their mode of formation. To some of our readers, it may be necessary to state what and where these Roads are. The deep natural cavity along which the Caledonian Canal passes, is called the Great Glen of Scotland. Several glens open into it, and among these is the glen through which the river Spean flows, ten miles northward of Fort William. Glen Roy is a lateral branch of Glen Spean, about eleven miles long, one mile broad, and pretty steep in the sides. The Spean falls into the river Lochy near Loch Lochy, and the Roy falls into the Spean five miles eastward. On both sides of Glen Roy, there are *three narrow terraces of gravel and clay*, called "the Parallel Roads." They project a few feet or yards from the sloping side of the mountain, forming three slightly marked lines along the valley from end to end, and exactly parallel to each other. At some parts they are entirely obliterated; for instance, when they cross bare rock, where loose matter would not rest, and when the surface of the hill is gently inclined, because their slope there coincides with that of the alluvial cover. Very often their appearance is so faint, that a spectator may find himself standing on one without being aware of its existence; but at other parts they swell out into pretty broad terraces; and what seemed obscure to a spectator in juxtaposition with it, becomes more distinct when seen from the opposite side of the valley, where the eye takes in the line for a length of some miles, and the obscure parts and the well marked parts are seen in connexion. Each of these shelves or terraces has been found by levelling to be perfectly horizontal, both lengthwise and on the opposite sides of the valley. In the annexed figure, let *h h* represent a portion of the surface of the hill, consisting of rock, with a layer of clay and gravel resting upon it, as indicated by the uneven line. The Parallel Roads consist of thicker portions of this alluvial cover, projecting slightly like a shelf or terrace.

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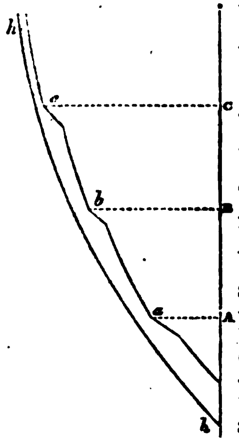
[*] This notice of the Parallel Roads of Glen Roy, which we have slightly altered, is taken from a paper on the subject, lately published in a widely circulated periodical, the Scotsman, by a gentleman well known to the geological world.



The *first* or lowest terrace *a*, is 972 feet above the level of the sea. The *second*, *b*, is 212 feet higher, or 1184 feet above the sea. The *third*, *c*, is 182 feet higher than the second, or 1266 feet above the sea.

The shelves slope inward to the centre of the valley, (to the line C, B, A).

In the adjoining valley of Glen Gloy, separated by an isthmus of high ground half a mile broad, there is a terrace twelve feet higher than *c*, or 1278 feet above the sea, but none lower. There is a remnant of a similar terrace at Kilfinnin, four miles northward, which is forty feet higher than *c*, or 1306 feet above the sea. The three terraces of Glen Roy follow the line of the valley down to its junction with Glen Spean, passing into all its sinuosities and recesses. Here two of them disappear; but the lowest continues its course up to the head of Glen Spean, and down nearly to its mouth, over a space of twenty miles, always preserving the same level. Lastly, portions of terraces have been discovered on the sides of the valley of the Spey, many miles eastward of those we have been describing, and at a computed elevation of 800 feet above the sea.

The Memoirs of Sir Thomas Dick Lauder and Dr MacCulloch,[*] written nearly at the same time, prove that the terraces were formed by water. In lochs which have pretty steep banks, covered with materials somewhat loose, rains and storms hurry down earth and stones, which have their motion checked when they reach the water, and there form a projecting ledge or shelf, which is nearly on a level with its surface close to the shore, but slopes downwards as it advances, till it is some feet or yards below the water at its outer margin. Sir Thomas Dick Lauder observed shelves of this description under the water in Loch Lochy, Loch Oich, Loch Ness, and also in Linnhe Loch, which is an arm of the sea, and he inferred, that if by any accident one of these lochs were drained, the shelf round its margin would exactly represent one of the parallel roads of Glen Roy. The inference was established, and the principle beautifully illustrated, by a parallel case. At Subiaco in the Apennines, forty miles from Rome, there is a small valley, with a terrace of gravel and clay round its sides, similar to those of Glen Roy. The stream flowing through it escapes by a fissure in the rock at its lower end; but the Roman writers tell us that it was anciently a lake, and the fact is proved by the remains of baths yet standing, and precisely at the level of the terrace. Whether the fissure in the barrier, which is twelve or fifteen feet wide, was opened by an earthquake, or cut by the water, is not known.

Sir Thomas supposes that Glen Roy, Glen Gloy, and Glen Spean, formed distinct lochs, each being closed in by a barrier at its lower end. The barrier at the foot of Glen Roy had kept the water for a long period at the level of the dotted line *c* C, till the alluvium collected and formed the shelf *c*.

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[*] Transactions of Royal Society of Edinburgh, vol. ix., and of Geological Society of London, vol. iv. first series.

The barrier was then partially removed or breached, and the waters subsided to the level *b* B, where they continued stationary till a new deposit of alluvium collected at the margin of the loch, and formed the shelf *b*. The barrier was again lowered or breached; the waters subsided to the level *a* A, where they remained stationary till the shelf *a* was formed. When this took place, the valleys of the Roy and the Spean formed one loch; for the shelf *a* passes from the Roy into the Spean on the same level, and extends along both sides of the latter for a space of 20 miles. On the other hand, no trace of the higher shelves *b* and *c* are found in the valley of the Spean. Sir Thomas therefore supposes, that when this lowest shelf *a* was formed, the two lochs were united, and shut in by a common barrier, which was below the junction of the valleys, about Highbridge, near the Great Glen. This barrier in its turn gave way, and the waters subsided gradually to their present level, leaving both valleys dry, and merely traversed by rivulets.

Glen Gloy in the same way formed a separate loch, shut in by a barrier near Loch Lochy, which held up the waters 12 feet above the greatest height of those in the Roy, till the shelf existing there was formed; after which the barrier opened, and the waters subsided till the valley became dry, without forming any other shelves.

Dr MacCulloch's theory is similar to the above, but he assumes the existence of one or two barriers only, instead of three.

The insuperable objection to both theories is, that no trace of the barriers is visible. They must have consisted either of walls of solid rock, or mounds of clay and gravel, stretching across a valley one, two, or three miles wide; and whether we suppose that they were burst through at once from top to bottom, or worn down by successive stages, it is utterly inconceivable that every trace of such walls or mounds could have disappeared. If the nature of the process left this doubtful, the analogous cases of the lake of Subiaco, and of the Swiss lakes which have burst their barriers, would prove the fact. Neither can we assume that a transient deluge had swept away the remnants of the barriers; for a deluge sufficient to accomplish this would have obliterated every trace of the shelves. We see, besides, how the problem is complicated by the additional facts successively brought to light. Had the terraces been found only in Glen Roy, one barrier would have sufficed for the theory, and its existence, though no vestige of it remains, might have been admitted. But the terraces were found in Glen Spean, and another barrier must be imagined there against all probability. They were found again in Glen Gloy, differing in level from those of the Roy, and thus a third imaginary barrier is rendered necessary. But the difficulties are not yet at an end. Mr Darwin finds terraces at Kilfinnin; of course a fourth barrier must be assumed here; and terraces having been found in the valley of the Spey, a fifth is equally required. Now, it is extremely improbable that even one of these barriers should have disappeared, and left no wreck behind; but that all the five should have vanished, and not a vestige of any one of them remain, is utterly inconceivable.

Mr Darwin attributes the formation of the terraces to the sea, which at an early epoch filled these valleys, as it now fills the cavities forming Loch Eil, Loch Etive, Loch Long, &c. He thinks that the upheaval of the land, followed, of course, by the retreat of the waters, took place slowly and gradually, as in Sweden at this day, not constantly and equably, however, *but with intermissions of rest, during which the terraces were formed*—that, in fact, these terraces belong to the earlier stages of that great series of movements by which the mainland of Scotland was raised above the water, and of which the last stage is so distinctly marked by the rise of thirty feet in the bed of the Forth.

As geologists have long been aware that the highest mountains have at one time been covered by the sea, this explanation must have occurred to others, who were perhaps led to abandon it by two apparently formidable objections,—first, the entire absence of marine remains from all the terraces; secondly, their local character, their want of continuity, and their non-existence in hundreds of other localities, where they should have been found, on the supposition that the cause which produced them was of so general and comprehensive a character. It would be too much to say that Mr Darwin has removed every difficulty which attends his hypothesis, but we think that his answer to the objections is upon the whole satisfactory.

The absence of marine remains is accounted for by the tendency of such animal substances to decay. On the coast of Forfarshire Mr Lyell found sea shells in gravel beds extending to the height of fifty or sixty feet, but in similar deposits of gravel, at greater altitudes, he found none, doubtless because the higher beds being first out of the water, and longest exposed, the organic remains in them had been decomposed. In Norway, near Christiana, there are stratified deposits of clay and sand ascending to the height of 600 feet above the sea; but shells are only found in those patches which are under 200 feet, and they are rare, and generally much decomposed, in those whose height exceeds 50 feet.

Mr Darwin, after citing these and other similar facts, adds, "In the extensive and superficial beds of elevated shells on the coast of Peru, where rain does not fall, and where consequently loose matter is not washed from the surface, I have traced, as I have ascended from the beach, a most perfect gradation in the decay of the shells, until a mere layer of calcareous powder without a vestige of structure alone remained."

Sweden is rising from the sea at the rate of three feet in a century. If we suppose the elevation of the soil of Scotland to have taken place at the same rate, the 900 feet of difference of level, between the Forfarshire beds which contain shells, and the lowest terrace in Glen Roy which contains none, will correspond to a period of 30,000 years; and from the facts just cited, we may safely infer, that a much shorter exposure would suffice to obliterate every trace of organic remains from the terraces, supposing that they originally existed there.

As to the second objection, the non-continuity or entire absence of the shelves, Mr Darwin's argument involves many details, and we can do little more than allude to it. He shews that a combination of circumstances, perhaps rarely occurring, was necessary to produce them. Among these are, a considerable inclination of the surface, the absence of bare rock, the existence of moveable debris above, the non-existence of strong currents. Add to this, that rains and storms must have had a considerable effect in obliterating the shelves; and that they must escape notice in many cases where they do exist, from the faintness of their appearance, and because they were not looked for. When Mr Darwin's speculations become known, we have no doubt that remains of terraces will be found in a hundred places where their existence has never been suspected. He accounts for the non-appearance in Glen Spean of the two upper terraces of Glen Roy, by observing, that when the water stood at the level of these terraces, Glen Spean would communicate with Glen Spey, and being then a channel connecting opposite seas would be traversed by strong currents. He shews that a slight variation of circumstances makes any of the three terraces in Glen Roy disappear for a considerable space; that an intermediate terrace between the second and third once existed, of which only a small remnant is discoverable; that on the supposition of the waters receding slowly, an indefinite succession of little terraces would be formed, which would blend with the alluvial cover, and be undistinguishable from it, except at some parts where conditions very favourable to the generation of terraces existed; that the existence of a terrace in Glen Gloy twelve feet above the highest in Glen Roy, may be explained either in this way, or by the different height of the tides, as in the Strait of Magellan, where the tide rises twenty feet at one part, and forty at another some miles farther on. The sum of his argument is expressed in the general remark, that the obliteration of the terraces may be regarded as the natural and usual result—their preservation as an exception, arising from peculiar and favourable circumstances.

Erratic blocks belonging to distinct localities are found both imbedded in, and resting on, the terraces. He supposes that masses of floating ice caught up these stones, and having stranded on the terraces, dropt them there. He found some at an elevation of 1000 feet above the upper terrace in Glen Roy, shewing that the water had once stood at that height. His remarks on the water-worn isthmuses connecting the valleys of the Roy, the Spey, and the Gloy, on the buttresses of gravel at the tributary streamlets, and on the deep alluvium in the bottom of these valleys, we have not room to speak of.

Mr Darwin's memoir connects together, and furnishes a key to a vast number of phenomena. In looking back to our own notes, taken on a visit to Glen Tilt, in 1828, we find various facts which the principle he has evolved may serve to explain. We find, for instance, terraces of gravel marked as existing at the following localities: 1. In the open country between Perth and Dunkeld, forty or fifty feet above the waters of the Tay; 2. At Logierait, a large table-shaped terrace, sixty or seventy feet above the flat land bordering the river, and probably 300 feet above the sea; 3. At Auld Clunie, above Killiecrankie, terraces at various heights—from 150 to 400 feet above the river, and probably from 400 to 700 above the sea; 4. In Glen Tilt, near Glen Criny, remnants of terraces ascending to the height of 100 to 150 feet above the stream, and 750 or 800 above the sea. On the top of these last, and on the south side of the stream, we found boulders of granite of one or two tons weight, belonging either to the rock on the opposite side of the deep ravine, or to some more distant locality. These alluvial terraces have generally deep cuts or gullies in them, displaying their structure and materials. Even the lowest ones, such as those between Perth and Dunkeld, can only be well accounted for by assuming, that the Tay formerly fell into a sea which stood forty or fifty feet higher than that which now receives its waters.

The perfect horizontality of the terraces of Glen Tilt and Glen Spean, prove that the levels were not sensibly disturbed over a space of twenty miles square, by the upheaving movement. Judging from the case of Sweden, we may even suppose that the rise was simultaneous, and nearly equable over all Scotland. We may hence infer, that when the upper terrace of Glen Roy was formed, Scotland consisted of an archipelago of islands, with one broad firth in the middle, and numerous narrow straits, canals, and inlets traversing its southern and northern divisions.*

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* Notwithstanding the strong case Mr Darwin and his geological friend have made in support of their speculation, we are not disposed to go along with them, but rather to consider the Glen Roy and other similar terraces, as formed by the emptying of salt or fresh water lakes. The objections to the lake hypothesis, offered by some geologists, are not valid, owing to the subject being viewed by them on the small scale.—Edit.

Proceedings of the Society for the Encouragement of the Useful Arts for Scotland, Session 1838-9. (Continued from Vol. xxv. p. 413.)

The Society for the Encouragement of the Useful Arts held the first meeting of its eighteenth Session in the Royal Institution, on Wednesday, 14th November 1838, Sir John Graham Dalyell, President of the Society, in the chair.

Sir John, on taking the chair, addressed the meeting to the following effect:—The Society had now assembled after a short recess. The multiplied business of the preceding session left little interval for leisure. A greater number of valuable papers had been read, a greater number of machines, models, and drawings, of expedients and improvements in the arts presented, than in any previous year; fifty-four new members also enlarged the roll. The views of the Society being directed exclusively to the public benefit, no one was denied its patronage. It was impossible to define the importance of the useful arts. That might be the subject of a long discourse. But as from these almost every human comfort and convenience were derived, it could be estimated only by anticipating the condition of mankind if deprived of them; for an immediate relapse to the savage state would follow. Bereft of the arts, man is a wanderer, naked and defenceless, seeking a precarious subsistence, inhabiting the woods or caverns in the earth, ignŏrant of divine and moral precept. Possessed of the arts, we revel in luxuries, dwell in palaces, roll in elegant carriages, clothe ourselves in silks and satins; and, above all, we ascend to the regions of science through the subservience of the arts. The vast importance of patronizing the useful arts was hence self-evident; it needed no demonstration. But however zealous in its duty, the Society itself required the public countenance, for the common cause could not be rendered sufficiently effectual by its labours and resources alone, though the grateful recompense of approbation was a powerful stimulus to renovated exertions.

The following communications were then laid before the Society:—

1. **On Geodetical Surveying and Trigonometrical Levelling. By the**

Rev. William Galbraith, M. A., Teacher of Mathematics. (586.) Thanks voted. To be printed in the Transactions.

2. Letter from Dr James Hunter, M. S. A., with specimens of writing paper, from which all traces of writing executed with Dr Traill's "Indelible Ink" have been effaced by water, applied with a camel-hair pencil, although that ink is known to resist the most powerful chemical agents; also, Letter from Dr Traill on that subject, with written specimens and a bottle of his ink, shewing an opposite result. (580.) Referred to a Committee to experiment with this ink, and different qualities of writing paper, and to report. In the course of the discussion on this interesting subject, several members brought under the notice of the Society the very different chemical qualities of different sorts of paper, into the composition of which many kinds of foreign ingredients, extremely prejudicial to the paper as well as the ink, were too often improperly introduced, such as acids, alkalis, earths, &c.; and that this, as well as a difference in the qualities of the inks used, might account for the different results obtained by those whose communications had been read this evening.

3. On a Method of obviating the consequence of the Expansion and Contraction of the Rails of Railways; with a Drawing. By Mr D. Hope, 34 Buccleuch Place. (584.) Referred to a Committee.

4. Donation.—Report of the Directors of the Manchester Mechanics' Institution, 1838. (577.) Thanks voted.

5. Donation.—The Economy of a Coal-field. An Exposition of the objects of the Geological and Polytechnic Society of the West Riding of Yorkshire, and of the best means of attaining them. By James F. W. Johnston, M. A., F. R. SS. L. and E., Assoc. Soc. Arts. Printed at Durham 1838, at the request of that Society. From the Author. (585.) Thanks voted.

6. Donation.—Astronomical Observations made at the Royal Observatory, Edinburgh. By Thomas Henderson, F. R. S. E. and R. A. S., Professor of Astronomy in the University of Edinburgh, and her Majesty's Astronomer for Scotland. Vol. I. From October 1834 to December 1835. Published by order of Government. Edinburgh, 1838. From the Author. (578.) Thanks voted.

The following gentlemen were admitted Ordinary Members, viz.—

Mr George Wilson, of Summerlee Ironworks, West Thorn, near Glasgow; John S. Patrick, Esq. of Trearne; Dr H. W. Nachot, Edinburgh; and Mr Alexander Aitken, ironmonger, Edinburgh.

The Society appointed the following Committee of Twelve Ordinary Members, to award the Prizes for Session 1837–8, viz.—

Sir John Graham Dalyell, kt., President; John Dunn, G. L. Finlay, Dr Fyfe, Richard Hunter, Robert Maxton, Walter Nicholl, James Slight.

Wilkinson Steele, David Stevenson, Richard Whytock; Dr David Maclagan, *Convener* the Secretary *ex officio*.

The Society elected their Office-Bearers for Session 1838–39, viz.

THE QUEEN, *Patroness.*

SIR JOHN GRAHAM DALYELL, KT. F. A. S., *President.*
ANDREW FYFE, M. D., F. R. S. E. } *Vice-Presidents.*
JOHN SCOTT RUSSELL, M. A., F. R. S. E. } *Vice-Presidents.*
JAMES TOD, W. S., 21 Dublin Street, *Secretary.*
MUNGO PONTON, F. R. S. E., 30 Melville Street, *Foreign Secretary.*
JOHN SCOTT MONCRIEFF, Accountant, 63 Queen Street, *Treasurer.*
JOHN DUNN, 63 Hanover Street, *Curator of Museum.*

Ordinary Councillors.

JAMES MILNE.
RICHARD WHYTOCK.
JAMES SLIGHT.
ROBERT BALD.
Professor FORBES.
Rev. WM. GALBRAITH.
ALEXANDER BLACK.
Dr MACLAGAN.
Sir JOHN ROBISON, K. H.
RICHARD HUNTER.
GILB. L. FINLAY.
JOHN BELL junior.

28*th Nov.*—Richard Hunter, Esq. in the Chair. The following communications were laid before the Society—

1. Additional verbal notice on the Properties of certain Forms of Buildings, intended for a public audience. By John Scott Russell, M. A., F. R. S. E., and Vice-President Society of Arts. (587.)—Thanks voted.

2. Notice of an Erroneous Method of using the Theodolite, with a strict investigation of the effects of different arrangements of readers. By Mr Edward Sang, F. R. S. E., civil-engineer and machine-maker, Edinburgh. (592.)—Thanks voted, and the paper to be printed.

3. Read and approved the Report of the Committee on the communications of last Session regarding the Feeding of Steam-engine Boilers with Water, viz.—

1. Mr Comin's. (474.)
2. Mr Carrick's. (506.)
3. Mr Mudie's. (509.)
4. Mr Combe's. (529.)
5. Mr Rose's. (537.)
6. Mr Russell's. (543.)
7. Mr Gall's. (548.)
8. Mr Bald's. (549.)
9. Mr Whitelaw's. (553.)

4. Read and approved the Report of the Committee on Mr Proudfoot's Machine for sharpening and setting the Teeth of Saws, read last session. (541.)

5. Dr D. B. Reid reported progress as to the proceedings of the Committee for promoting an Exhibition of interesting natural and artificial objects, models of machinery, &c.; and it was suggested that he, as Convener, should apply to the Highland Society, and to Professor Low, for their countenance and support to the object in view, by allowing some of their models of machinery and implements to form part of the collection on that occasion, which is expected to be a large and interesting one.

The following Candidates were admitted as Ordinary Members, viz.—

James Mackenzie, Esq. W. S. 10 Ainslie Place; John Burn Murdoch, Esq. of Gartincaber, advocate; Mr Thomas Jameson, 47 London Street; Mr John White, teacher, 3 North St David Street; Mr George Wauchope, merchant, 21 South Bridge Street; Henry Graham, Esq. surgeon, 11 Shandwick Place.

12*th Dec.*—Sir John Graham Dalyell, Kt. President, in the Chair. The following Communications were laid before the Society:—

1. Notice of an improved method of combustion, successfully practised in England, whereby a considerable saving is effected, and the smoke entirely consumed. By John Scott Russell, M. A., F. R. S. E., V. P. Soc. Arts. (600.)—Thanks voted. This notice excited great interest;—Mr Russell stated that he had seen this method in operation, and from inspection of the books for a course of years, he had satisfied himself that the *actual* saving of fuel was to the extent of 18 per cent. besides burning the smoke.

2. Notice of an indelible ink, invented by P. D. Veitch, M. D., No. 8 Hart Street, Edinburgh. Communicated by James Hunter, M. D., M. S. A. A bottle of the ink, and some specimens of writing executed with it, were exhibited. (599.) Referred to a Committee. An interesting conversation then took place with regard to indelible inks in general, and writing papers of different composition, in which Dr Hunter, Dr D. B. Reid, Dr Maclagan, Mr Ponton, and others took part. Dr Hunter stated some other methods of preventing the effacing of writing which had occurred to him; and of which he promised to give a written notice on a future occasion.

3. On an artificial method of augmenting the Tone of the Violin. By Mr John St Clair, Teacher of Music, 79 South Bridge Street, Edinburgh. The instrument was exhibited. (588.) Referred to a Committee.

4. Donation.—Sketch of the Civil Engineering of North America; comprising Remarks on the Harbours, River and Lake Navigation, Lighthouses, Steam Navigation, Water-works, Canals, Roads, Railways,

Bridges, and other works in that country. By David Stevenson, Civil Engineer, M.S.A. 1838. From the Author. (594) Thanks voted.

5. Donation.—Account of the Invention of the Pantograph, and a Description of the Eidograph, a copying instrument invented by William Wallace, A.M., F.R.S.E., &c., Professor of Mathematics in the University of Edinburgh. From the Transactions of the Royal Society of Edinburgh, Vol. XIII. From the Author. (595.) Thanks voted.

Mr F. E. Seeligmann, Punch-cutter, Edinburgh, was admitted an Ordinary Member.

26th December.—Annual General Meeting, for the purpose of awarding Premiums. Sir John Graham Dalyell, President, in the chair. The following communications were laid before the Society:—

1. The Report of the Prize Committee, awarding the Prizes for Session 1837-8 was read; and the Prizes were delivered by the President to the successful Candidates.

1. To Mr EDWARD SANG, F.R.S.E.,—Civil Engineer and Machine Maker, Edinburgh, for a Notice of a Dioptric Light erected at Kirkaldy; with a Description and Drawings of the Apparatus used by him for cutting the Annular Lens to the true optical figure;—read, and the apparatus exhibited, 25th April 1838:—printed in the Society's Transactions, 1838. (552.)
   The KEITH *Medal, value Twenty Sovereigns.*

2. To Mr JAMES WHITELAW, 18 Russell Street, Glasgow, for a Description, with Drawings, of a Grinding Machine, which is used instead of a Turning-lathe for giving a truly cylindrical form to the rims of Pulleys and Drums; and more particularly for a Drawing and Description of a Machine invented by him for grinding Pulleys round on the rim;—read and exhibited 14th February 1838;—and printed in the Society's Transactions, 1838. (512.)
   The Society's Silver Medal, value Ten Sovereigns.

3. To JOHN ALSTON, Esq. Hon. M.S.A. Honorary Treasurer to the Glasgow Asylum for the Blind, for Specimens of a series of Fables, with Woodcut Illustrations, then in progress of being printed by him for the use of the Blind at Glasgow;—exhibited 30th May 1838, (568.):—Also for his Musical Catechism, with Tunes for the use of the Blind, printed in raised Characters at the Glasgow Blind Asylum;—exhibited 16th May 1838, (559.);—and, generally, for his zealous, energetic, and benevolent exertions for the education of the Blind.
   The Society's Silver Medal, or a piece of Plate, value Ten Sovereigns.

4. To Mr GEORGE WILSON, 77 Broughton Street, Edinburgh, for his Model sent in competition for the Prize offered for "a convenient and simple method of increasing or diminishing the distance of the Floats from the centre of the common Paddle-

wheels, during the motion of the Vessel, so as to adapt them to change in the load and draft of water;" exhibited 30th May 1838.—(524.) Although the Society do not hold any of the competing Papers on this subject as altogether attaining the object proposed in offering the Prize, or that they would be justified in recommending any of the proposed Plans to be adopted in practice; yet the Committee have awarded a Prize to Mr Wilson for his ingenious Model, and as a reward for the mechanical contrivance it displays.

The Society's Silver Medal, value Five Sovereigns.

5. To Mr John T. Rose, 1 Prince Regent Street, North Leith, for the ingenuity displayed in his communication on the same subject;—read 30th May 1838; and to encourage him to persevere in mechanical pursuits. (538.)

The Society's Honorary Silver Medal.

6. To Mr Robert Mudie, late of 14 Fountainbridge, Edinburgh, now in Australia, for the ingenuity and attention to the subject displayed by him in his Drawing and Description sent in competition for the Prize offered for "a convenient mode of filling the Boilers of Steam-Vessels with water, while the Vessel is at rest, so as to remove a frequent cause of explosion;"—read and exhibited 11th April 1838. (509.)

The Society's Honorary Silver Medal.

7. To Mr John T. Rose, 1 Prince Regent Street, North Leith, for the ingenuity displayed by him in a communication on the same subject, of which the Drawing and Description were read and exhibited 11th April 1838. (537.)

The Society's Honorary Silver Medal.

N.B. With regard to these two Plans, the Report of the Committee to whom they were remitted states, "Mr Mudie's appears fully the better, but both are so complicated, that your Committee cannot recommend them for adoption; at the same time, they think that the ingenuity and attention to the subject displayed by these gentlemen (both young men) is deserving of the favourable consideration of the Society."

8. To William Alexander, Esq. W. S., M. S. A., Edinburgh, for his ingenious Model and Description of his Electro-Magnetic Telegraph;—read and exhibited 15th November 1837. (489).

The Society's Honorary Silver Medal.

The Committee have awarded an Honorary Medal to Mr Alexander for the ingenuity displayed in the construction of the Working Model of the Telegraph exhibited by him and for being the first person who, in Scotland, brought forward the subject in a tangible form.

9. To Mungo Ponton, Esq. F.R.S.E., late V.P.S.A.,—for the ingenuity displayed by him in the Model and Description of his Improved Electric Telegraph;—read and exhibited 10th January and 20th June 1838, when Mr Ponton presented his elegant Model to the Society, to be placed in their Museum. (492.)

The Society's Honorary Silver Medal.

10. To John Scott Russell, M.A., F.R.S.E., and V.P.S.A., for Drawings and Description of the best Method of Constructing the Interior of Buildings intended for the accommodation of Auditors and Spectators, and its application to Lecture Rooms, Churches, and Music Rooms; by arranging the seating according to certain Isacoustic and Iseidomal Curves;—read 16th May 1838, (542), and ordered to be printed in the Transactions.

The Society's Honorary Silver Medal.

11. To Charles Heath Wilson, Esq. R.I.A. Assoc. Royal Scot. Acad., 8 Northumberland Street, Edinburgh, for his Essay on the Expediency of forming National Establishments for Moulding and Casting Works of Art, with Observations on the improvements which would result from the adoption of such a measure;—read 28th March 1838. (555.) Printed in the Society's Transactions 1838.

The Society's Honorary Silver Medal.

Your Committee beg leave to report, that there has been no competition for the following Prizes:

2d Prize, Society's Silver Medal value Ten Sovereigns.
3d Do. ———————— Five ————
5th Do. ———————— Five ————
6th Do. ———————— Five ————
11th Do. The Society's Honorary Silver Medal.

Before concluding their Report, your Committee beg to propose, that,—while Thanks are due to all those Gentlemen who have sent Communications to the Society,—the *special* Thanks of the Society should be given to the following Gentlemen for their respective Communications, viz.:—

1. To Robert Bald, Esq. F.R.S.E. Mining Engineer, Edinburgh, for his Plan for Feeding Marine Steam-Engine Boilers with Water;—exhibited 11th April 1838, (549.) which the Committee reported "to possess all the advantages of the other plans, and none of the complication, and that it could be used with advantage, only it is not a self-acting feeder, as it requires the attention of the engineer—but it is exceedingly simple." This Communication was not a competing one.

2. To John Scott Russell, M.A., F.R.S.E., V.P.S.A., for his Communication on the same subject;—read 11th April 1838; (543.)—which was almost identical with the preceding, and of

which the Committee reported in equally favourable terms. This Communication, also, was not a competing one.

3. To William Galbraith, M.A., C.S.A., Teacher of Mathematics, Edinburgh, for his Communication on a Formula to obtain the Decrease of Temperature according to the Height above the Earth's Surface;—read 11th April 1838. (546.)
4. To Mr Edward Sang, F.R.S.E., Civil Engineer, Edinburgh, for his Essay on the Construction of Oblique Arches; with Model and Drawings;—read and exhibited 16th May 1838, and previous dates. (511.) Ordered to be printed in an abridged form in the Transactions.
5. To Mr George Richardson, Printer, 35 Miller Street, Glasgow, for a Communication sent by him in competition for the Prize offered "For a Method of Removing the Plaster of Paris from the Types after Stereotyping, without injuring the Types;" —read 28th February 1838. (516.)

The Thanks of the Society are given to Mr Richardson because he has to a certain extent succeeded;—in so far as he has found out a simple method of removing the Stucco;—but the condition of the Prize is, that this must be done "without injuring the Types;" now, it is found that the Types, after having undergone his process, become, when dry, so firmly cemented together, that they cannot be separated except by force, which is not only attended with injury to the Types, but with greater delay than the common mode. A method of preventing the latter defect would render his invention eminently useful.

All which is humbly reported by

DAVID MACLAGAN, *Convener.*

Museum of the Society of Arts,
63 Hanover Street, Edin.
10*th December* 1838.

In presenting the "*Keith Medal*" to Mr Sang, Sir John made a few remarks to the following import.—"The late Alexander Keith, Esq. of Ravelston and Dunottar, the representative of an ancient and honourable family, often celebrated in Scottish history, had laudably promoted the cause of science while he lived, and had devised the means of its advancement after his decease, now at disposal of the Society, as the 'Keith Medal,' to be given for the most important invention, discovery, or improvement in the useful arts. But no opportunity had hitherto occurred since the Keith Fund came into the possession of the Society of awarding that prize. Now, however, it was highly gratifying to find one of the most scientific, useful, and meritorious of all who had been connected with the Society, Mr Edward Sang, entitled to this eminent distinction. His skill, his labours, and unermitting exertions in various scientific departments, were too well known to be embellished by any commentary. It was alike creditable to him to receive such a testimony, and for the Society to have such an opportunity of bestowing it."

2. The Models, Drawings, &c. of Inventions, &c. (Session 1837-8), for which Prizes have been awarded, were exhibited.

3. Part first of a paper on a method of obtaining the greatest possible exactitude from the data of a Survey, by Mr Edward Sang, F.R.S.E., M.S.A., civil-engineer and machine-maker, Edinburgh, was then read, for which Mr Sang received the thanks of the Society. (596.)

4. Specimen of a new Method of Printing in Relief for the use of the Blind, by Messrs Leith and Smith, Lithographers, Hanover Street, Edinburgh, was exhibited. (604.)

5. Donation.—The Principles of English Grammar, in raised Letters, for the use of the Blind. Glasgow, printed at the Institution Press. By John Alston, Esq. Honorary Treasurer to the Asylum for the Blind. 1838. From John Alston, Esq. (605.)

6. Donation.—A Selection of Fables, with Wood-cuts, for the use of the Blind. Glasgow, printed in the Asylum, by the Institution Press. 1838. From John Alston, Esq. (606.)

7. Donation.—The Fifth Annual Report of the Managers of the Pennsylvanian Institution for the Instruction of the Blind. Philadelphia. 1838. From John Alston, Esq. (607.)

8. Donation.—An Account of the Hospital and School for the Indigent Blind at Norwich. (1838.) From John Alston, Esq. (608.)

The thanks of the Society were voted to Mr Alston for these four donations.

At the conclusion of the public business of this evening, the following motion was made by David Maclagan, M.D., F.R.S.E., and seconded by Sir John Graham Dalyell, kt., President of the Society:

Dr Maclagan, after expressing the high sense which the Members unanimously entertained of the services of the Secretary, in forwarding the objects and promoting the prosperity of the Society, moved, That an especial vote of thanks be given to Mr Tod, for the great ability and unwearied zeal with which he had so long discharged the duties of Secretary, and which Dr Maclagan felt assured, the Society would consider it to be their duty to acknowledge by some further mark and memorial of their approbation.

Sir John Graham Dalyell, in seconding the motion, expressed his entire concurrence in the opinion entertained of Mr Tod's merits, and proposed that it be remitted to the Council of the Society, to consider and report in what manner the object of the motion could be best carried into effect; which was agreed to unanimously.

16*th January* 1839. Sir John Graham Dalyell, the President, in the chair. The following communications were laid before the meeting:—

1. Part Second of a paper on a Method of obtaining the greatest possible Exactitude from the data of a Survey. By Mr Edward Sang,

F. R. S. E., M. S. A., civil-engineer and machine-maker, Edinburgh. (596.)

2. On the Vibrations of Suspension Bridges, and other Structures, and the means of preventing Injuries arising from this cause. By John Scott Russell, M.A., V.P.S.A. (590.)

Mr Russell's attention had been directed to this subject, so important to the arts, by having occasion some years ago to have a structure of timber of great height and exceedingly slender, erected for experimental purposes. The evils arising from oscillations had in that case been strictly inquired into, and had led to the examination of the principles of those oscillations and the best and simplest mode of obviating them. The case of Suspension Bridges, was that to which these principles were most directly applicable, both on account of their extreme lightness and slenderness, and because their strength was so near the limits of possible cohesion that mere weight of materials would not in that case, as in most others, supply the place of advantageous arrangement. The subject was at present rendered highly interesting by the circumstance that both the Suspension Bridges of Montrose and of Menai were, from this cause, impassable to her Majesty's mails, and public business was thereby much interrupted. It was also in our recollection how recently the Brighton Pier and other structures of a similar nature had suffered from the same cause. The paper consisted, first, of an account of the author's original experiments and observations on the oscillations of structures; second, of the philosophical principles on which they depend; third, of the application of these principles to Suspension Bridges and other structures. It was shewn by some beautiful experiments, that the tendency of such structures is to oscillate in equal portions like the string of a musical instrument in sounding harmonics, and that the great object in mechanical structures was to stop these vibrations, whereas in musical strings it was to continue and increase the vibrations, the principles were the same, but the plans to be followed were opposite. It was, last of all, shewn, that, by placing stays in certain positions rather than others, they might be so arranged as to give a bridge perfect stability. It appeared that two stays, placed according to the rule, would give as much stability as thirty-five in the common way, and that four stays would do as much as one hundred—thus effecting great economy in weight and expense, with great stability and strength.

The following were admitted ordinary members:—

David Smith, Esq. W.S., 3 Doune Terrace; Mr F. Watson, bookseller, 52 Prince's Street; David Hunter, Esq. of Blackness, 67 Queen Street.

30*th January*.—John Scott Russell, M.A., F.R.S.E., Vice-President, in the chair. The following communications were laid before the Society:—

1. On an Expeditious Method of temporarily, but securely, stopping

Leakage in Steam-boilers, particularly applicable to the Boilers of Steam-vessels while on a voyage. By Robert Hay, Esq. R.N. Communicated by Dr James Weir, M.S.A. A working Model of the apparatus was exhibited. (602.) Referred to a Committee.

2. On the composition of a very firm Lute for the joints of Steam-pipes, &c. By the same. Communicated by Dr Weir. The lute was shewn, and its composition given. (603.) Referred to same Committee.

These communications excited considerable interest; and the general impression seemed to be, that although only useful in peculiar situations, the method is valuable as an additional expedient, to be ready for emergencies.

3. On Assaying Copper, and other Ores, by Galvanism. By Martyn J. Roberts, Esq. of Wales. Communicated by Dr D. B. Reid, F. R. S. E. M. S. A. The apparatus was exhibited. (611.) Referred to a committee.

Some remarks were made on this process by Mr Ponton, who seemed to fear that the whole of the salt of copper might not be decomposed, and who mentioned arrangements by which he had decomposed the last particle of salt. Mr Roberts, however, stated that he had put the solution to the severest test, and had found none of the salt undecomposed; and further, that his new process had been adopted with much success in Wales, and was preferred to the old methods.

4. Description of a new Waving Bar for Engravers' Ruling Machines. By Mr Edward Sang, F. R. S. E., M. S. A., civil-engineer and machine-maker, Edinburgh. The Machine was exhibited in action. (615.) Thanks voted. Referred to a Committee.

The chief novelties in this apparatus were the substitution of a cylindric waving bar, easily removable, for the usual flat one, by the turning of which, changes in the pattern might be produced,—and a combination of levers for altering the depths of the undulations. Specimens of work done by it were distributed among the members.

5. Donation.—The authorized Version of the Psalms of David in Metre, embossed for the use of the Blind. In 2 vols. Glasgow; printed in the Asylum, by the Institution Press, 1838. From John Alston, Esq. of Rosemount, Hon. M. S. A. (609.) Thanks voted.

Archibald Gibson, Esq., Accountant, 63 Queen Street, was admitted as an Ordinary Member.

13*th Feb.*—Andrew Fyfe, M. D., F. R. S. E., Vice-President, in the chair. The following communications were laid before the Society:—

1. Description of a new machine for lifting Stereotype Moulds from the Types, and a new method of Baking the Moulds, &c. used in the Stereotype Foundry of Messrs Ballantyne and Co. Edinburgh. By Mr

Daniel Macpherson, 4 Charles Street, Edinburgh. The machine was exhibited. (610.) Referred to a Committee.

2. On Geodetical Surveying and Trigonometrical Levelling, No. 2. By William Galbraith, M. A., C. S. A., Teacher of Mathematics, Edinburgh. (613.) Thanks voted, and to be printed in the Transactions.

3. Notice of a Protecting Frame, to enable persons to clean the outside of windows without danger. By Mr John Baillie, 23 Cumberland Street, Edinburgh. Frame exhibited. (589.) Thanks voted, and referred to a Committee.

4. Mr Ponton, with the permission of the Society, laid on the table a communication from Dr Traill, on the subject of his Indelible Ink, which was referred to a Committee and to be read at next meeting.

5. Donation.—The Suburban Gardener and Villa Companion. By J C. Loudon, F. L. S., H. S., &c. London, 1838. From the Author. (471.) Thanks voted.

The following were admitted Ordinary Members, viz., 1. Mr John Mackie, mining-engineer, Alloa Colliery, Alloa. 2. Mr Laurence Hill jun., civil-engineer, Barlanark, near Glasgow. 3. Mr J. G. Greig, surgeon-dentist, 57 Castle Street. Captain Alexander Milne, R. N., was elected an Associate Member.

27th Feb.—Sir John Graham Dalyell, Kt. President, in the chair.

This meeting presented more than the usual interest, from the circumstance of having been fixed upon for the presentation of a piece of plate to Mr Tod, the highly valued Secretary of the Society. It had been for a considerable time the unanimous feeling of the members of the Society that their great and growing prosperity was chiefly owing to the indefatigable assiduity of the Secretary, his diligence and discrimination in the discharge of his duty, and the tact which he displayed in reconciling parties of different and sometimes opposite views, so as to unite them all in the promotion of the objects of this society. The President, Council, and Members, had accordingly carried the general wish into execution. On this evening, Sir John Graham Dalyell, supported by the office-bearers and a most numerous meeting of the members of the Society, presented the plate to Mr Tod, and in doing so, Sir John spoke as follows, with his usual animated kindness of manner and manly tone of moral feeling: He adverted to the flourishing state of the Society, and the numerous and valuable inventions and communications which its proceedings occasionally elicited. He ascribed much of its prosperity to the diligence, zeal, and activity of their most meritorious Secretary, Mr James Tod. For above ten years his gratuitous services had been unremittingly devoted for its benefit, and through their medium, for behoof of the public. Nor was this an easy task, considering the great variety of matters brought under view, and the copious details respecting them, of which the proceedings of this same evening offered an impressive example. The remarkable order and arrangement also preserved by Mr Tod were highly conducive to the common welfare; but, above all, his integrity deserved the warmest commendation. He (Sir John) would ever rank integrity among the first of human qualities. Who could be so estimable as an honest judge a trusty friend, or a faithful servant?—one in whom implicit reliance might be reposed, and one who would never betray confidence. As President of this Society, he had already had many opportunities of delivering the rewards of merit; but none were better bestowed than this elegant silver salver now given from the chair, thus inscribed;—"Presented by the Society of Arts for Scotland to James Tod, Esq., W. S., their Secretary, in testimony of their respect for his character, and their sense of his invaluable services in forwarding the objects and promoting the prosperity of the Society." In name of the Society, Sir John added, he was confident that so flattering a testimony of approbation would be carefully preserved by Mr Tod, and transmitted to his latest posterity—while, as an individual, he earnestly desired him all health and comfort and happiness for the long enjoyment of so precious a gift.

Mr Tod shortly and appropriately returned thanks for the honour which had been conferred on him. He considered the compliments that had been paid to his services greater than they deserved, and this token of their regard would prove a stimulus to his future exertions in promoting the usefulness and well-being of the Society.

The usual business of the evening was of an interesting and valuable description.

1. Mr Macpherson exhibited the method of lifting Stereotype Moulds from the Types with his new Machine, as requested at last meeting. (610.) Thanks voted.

This beautiful little machine was presented with one of the stereotype moulds in the actual process of being lifted, and one of the members of the Society, totally unacquainted with the process, was enabled by means of it, to perform one of the most delicate and difficult parts of the art of stereotyping, in the most satisfactory manner.

2. In connection with the former subject, Mr Claud Russell exhibited one of the earliest Stereotype Plates, executed by Gedd in 1742. Thanks voted.

3. The third communication was a Stove of a new construction, said to have all the properties of the Arnott Stove (radiation, equalization, and regulation of heat, and economy of Fuel), without the tendency, in weakly drawing Vents, to return smoke or carbonic acid gas into the room. This Stove can, in an instant, be converted into a cheerful open fire, and, at the expense of about a penny a-day, render an apartment, especially a bed-room, perfectly comfortable, night and day. Invented by William Kirkwood, plumber, Thistle Lane, Edinburgh. (623.) Thanks voted, and referred to a committee.

This Stove was exhibited as a great improvement upon Arnott's Stove. In certain circumstances, such as where there is an ill drawing vent, Arnott's Stove is said to have been found not to answer well; and where it does not succeed, it is highly dangerous to the health, and even the life of individuals. This Stove is intended to remedy these defects; it is exceedingly cheap, and is elegant in appearance. Mr James Simpson, advocate, who was present, stated that the Stove was originally invented for his use; he had Arnott's Stoves in some of his bed-rooms, where, in consequence of ill drawing vents, they had injured the health of individuals, and Mr Kirkwood had proposed to him to remedy these defects. He stated, that Mr Kirkwood had perfectly succeeded, and that his apartments now possessed the greatest comfort, with perfect safety, at an expense of a penny a-day. Mr Simpson farther stated, that in another of his rooms he uses an Arnott Stove, which answers there perfectly.

4. Description and Drawings of a new and self-acting Railway Crossing. By Mr John Meiklejohn, 39 Home Street, Edinburgh. Working models were exhibited. (612.) Thanks voted, and referred to a Committee.

It is well known that where railways cross each other, or diverge from one another, there is both danger and jolting at the crossing. This self-acting switch is intended to remedy these inconveniences, and a very neat model of the simple apparatus, contrived by Mr Meiklejohn, was exhibited.

5. On the proper method of disposing of Panes of Crown Glass in the Frames of Windows. By Sir John Robison, K.H., Sec. R.S. Ed.,—C.S.A. (616.) Thanks voted, and remitted to the Experimental Committee.

Sir John Robison had experienced the evil of the ordinary way of putting in panes of glass, having the convex side outwards. When the action of the wind was strong, as during the late storm, when its pressure was nearly 8 lb. on a square foot, the convex side was forced in and rendered concave, and in doing so was broken. It was, therefore, proposed that experiments on this subject should be tried by the Society, and Sir John recommended that the panes should be put in windows with the convex side inwards.

6. Specimens of Stained Glass, executed in a new style, under recent patents obtained by him. By Mr William Cooper, patent glass manufacturer, Picardy Place, Edinburgh, M.S.A. (405.) Thanks voted. The specimens were various, and very much admired.

7. A letter was read from Dr Traill on his Indelible Ink. The specimen formerly sent to the Society had been taken by mistake from a bottle marked imperfect. (617.) Thanks voted.

The Committee reported, that, after many experiments, they found that the ink was perfectly indelible, and indestructible, if written on proper paper, and that the proper paper was that which was unsized and porous, like common printing paper. It appears that some banks have adopted this ink for orders, letters of credit, &c. The Committee reported that Dr Veitch's Ink, though indelible, destroyed the fabric of the paper.

The following Candidates were admitted as Ordinary Members, viz.—

1. William Kennedy, Esq. W. S. 57 Northumberland Street. 2. Samuel Wright, Esq. of Nottingham, 17 St Patrick Square. 3. George Glover, Esq. surgeon, 19 Buccleuch Place.

13*th March.*—Sir John Graham Dalyell, Kt., President, in the Chair. The following communications were laid before the Society.

1. On the best means of Burning Gas for supplying Heat. By Sir John Robison, K. H., Sec. R. S. Ed.—C. S. A. (581.) Thanks voted.

The manner of heating by the flame of a mixture of gas and common air passing through a tube having a diaphragm of wire gauze, was, in this paper, strongly recommended to be used in various processes in the arts; particularly in the hardening and tempering of fine edge-tools and other articles of steel. No scaling or oxidation takes place, and the finest edge is preserved entire.

2. Observations on Boots and Shoes, with reference to the Structure and Action of the Human Foot. By Mr James Dowie, Boot and Shoemaker, 57 Frederick Street, Edinburgh, M. S. A. Specimens, Casts, and Drawings were exhibited. (621.) Thanks voted, and ordered to be printed in the Transactions.

This was a very interesting paper, shewing that, as the human foot becomes elongated to the extent, in many cases, of two-thirds of an inch, when under pressure of the body, as in walking, unless provision be made for the elongation of the boot or shoe along with the foot, the feet and toes must be cramped, their healthy action impaired, and, in many cases, stiffness and even distortion of the joints of the toes superinduced. This desideratum has been effected by Mr Dowie, in his elastic boots and shoes, by the introduction of an elastic substance in the sole and upper leather, which, while quite impervious to water, has the property of enlarging and contracting along with the foot, both when in motion and under pressure, and when at rest and free from the pressure of the body.

3. On a Perfect Parallel Motion for the Steam-Engine. By John Scott Russell, M. A.—F. R. S. E., V. P. Soc. Arts. (626.) Thanks voted, and referred to a Committee.

This was a very beautiful and scientific application of a mathematical proposition, to the mechanism of the steam-engine. By this method the piston rod is carried up and down in a straight line, which is mathematically correct; while, by the method of Watt, the piston rod is never at any one point carried in an absolutely straight line, and at top and bottom of the stroke it is very considerably deflected from the straight line, which gives rise to much friction and strain on the mechanism.

4. A short notice on the Constitution of Steel. By Samuel Brown junior, Esq., Haddington. Communicated by Mr Alexander Bryson, M.S.A. (628.) Thanks voted, and referred to the Experimental Committee.

This paper was intended to draw the notice of the Society to the question whether steel were a chemical compound of iron with its carburet, or a mechanical union of carbon and iron. Crystals of carburet of iron were exhibited; and it was proposed that experiments be tried, with a view to the elucidation of the question.

5. Donation.—The Annual Report of the Institution of Civil Engineers, for Session 1830. From the Institution. (624.) Thanks voted.

27th March.—Sir J. Graham Dalyell, Kt. President, in the chair. The following communications were laid before the Society:—

1. On the best method of burning gas for the purpose of illumination. By Sir John Robison, K.H. Sec. R.S. Ed.—C.S.A. (582.)

The object of this paper was to point out the most economical mode of burning gas for the purpose of giving light; by peculiar construction of the burners, and the proper size and fitting of the glass chimneys; and the disuse of obscured shades. It is found to be more economical, with any burner, to burn the gas to the full height it can attain without smoking. If a smaller quantity of light be wanted, it is better to use a smaller burner than to reduce the flame of a larger one. The best effect of an argand burner is attained when the holes are all of one size, so that the flame shall be of an equal height all round. The paper also described the method of burning gas in street lamps, pointed out by Sir John Robison, and now very generally used in Edinburgh, so as to prevent the moisture from being condensed on the inside of the globes, and rendering the light obscure.

The thanks of the Society were given to Sir John for this communication.

2. Description of a Substitute for Door Springs. By Mr John Gilchrist, 10 Middle Arthur Place. A model was exhibited. (631.)

This appeared to be an ingenious method of applying lever power, instead of springs, for the purpose of shutting, and keeping shut, doors opening either way, and by which the greatest power is obtained just where it is wanted, viz. when the door is shut.—Referred to a Committee.

3. Dr Fyfe, V. P. described Mr Talbot's process of Photogenic Drawing.

The process was exhibited, and specimens were produced by means of the Oxy-hydrogen Light. (632.)

Dr Fyfe made a few remarks on the properties of the compounds of silver, more particularly their being darkened by the action of light; so that paper besmeared with them, and exposed to light, becomes dark coloured. Hence the process of photogenic drawing. The compound employed by Mr Talbot is the *Chlorid,* but Dr Fyfe stated that he preferred the *Phosphate,* not only because it is extremely sensible to light, but also because it is applied more easily and uniformly to the paper. It gives a pale yellow tinge, so that it is thus easily known when the paper is equally spread over with it. The proportions he recommends are, a saturated solution of phosphate of soda, diluted with three, or sometimes four of water, and a solution of Lunar Caustic, diluted with six of water, the phosphate being first applied. Instead of these solutions, he sometimes besmears the paper with phosphate of silver, suspended in water, to which a little mucilage is added. Paper thus prepared is equally *sensitive* as that with the chlorid. Specimens of impressions taken from dried plants, from engravings, and from landscapes on glass, both on chlorid and phosphate paper, were then exhibited. Dr Fyfe next made a few remarks on the means recommended for *preserving* the impressions. He stated that he had failed in preserving them by the use of sea-salt, as recommended by Mr Talbot, and that as to the *iodide* of potassium, also recommended by him, he had found great difficulty in applying it successfully, owing to the ease with which it acts on the shaded part of the impression. This had induced him to try the preservation by preventing the transmission of that part of the solar ray which causes the blackening. This he attempted by covering the paper with a red or yellow colour. In some trials which he had made, he had so far succeeded that impressions covered with a faint tinge, by chrome yellow, had resisted the action of light. One of these he exhibited, which had been hung up for two days opposite the windows of a room with a south exposure, and another which had been exposed at a window for several hours, during two or three of which the sun was pouring its rays on the paper. In both of these the impression seemed to be little, if at all, affected. Dr Fyfe stated that he had great hopes that this method might be applied successfully in preserving the impressions. It was also mentioned that the light of the oxy-hydrogen blowpipe was sufficiently intense to produce the impressions, and after finishing his remarks, he shewed the method of doing so. A small dried leaf of the wild geranium was used, by which, in seven minutes, a distinct representation of the leaf was produced. Dr Fyfe also read a letter from Mr Dunn, the Curator, stating that he had that day succeeded in procuring an impression of a section of a piece of cane, by means of the solar microscope. The impression, which was on phosphate paper, was exhibited. Thanks given to Dr Fyfe from the Chair.

4. The Report of the Experimental Committee on Dr Traill's Indeli-

ble Ink (617), and on the Writing Paper most proper to be used with it, was read and approved. The Report is in the following terms:—

Report of Experimental Committee on Dr Traill's Indelible Ink:—

The Committee having met, and having heard the letter of Mr Ponton read, and having reconsidered the Report of the Committee on Inks, and also the experiments made by the Experimental Committee at last meeting, with papers of various texture and surface, furnished by Mr Cowan, beg to report, that the ink is indelible only on particular kinds of paper, and are therefore of opinion, that the ink may be recommended as indelible only when used with paper properly prepared for the purpose, and tested.

(Signed for the Committee)
John Scott Russell, Chairman.

5. The Report of the original Committee on Dr Traill's and Dr Veitch's Indelible Inks, was read and approved.

The following Candidates were admitted as Ordinary Members, viz.:—

1. E. J. Macbrair, Esq. S.S.C. 22 Duke Street; 2. Henry Atwood, Esq. 63 Northumberland Street; 3. Mr George Duncan, Bootmaker to the Queen, 55 Prince's Street; 4. Mr William Smith, Merchant, 1 Blair Street; 5. Douglas Maclagan, M.D., F.R.C.S., 129 George Street.

10*th April.*—Sir John Graham Dalyell, Kt., President in the Chair. The following communications were laid before the Society.

1. Biographical Notice of the late Mr William Blackie, optician, Upper Hermitage, Leith, with an account of the principal optical apparatus executed by him. Communicated by John Coldstream, M.D. Leith. Some of the lenses and other apparatus were exhibited. (622.)

Thanks voted to Dr Coldstream for the Memoir, and to Professor Forbes, for exhibiting some of the lenses, prisms, &c.; and the memoir was ordered to be printed in the Transactions.

2. On Photographic Etching. By Andrew Fyfe, M.D., F.R.S.E., Vice-Pres. Soc. Arts. (636.)

Before proceeding to describe the Photographic Etching, Dr Fyfe stated that, since last meeting of the Society, he had succeeded in preserving the Photographic impressions done with the phosphate of silver, by immersing them in a weak solution of ammonia in water, which, though it affected the colour slightly, yet prevented the further action of the light. He then made some remarks on the process of etching in the common way, and afterwards on another mode of etching, by using a transparent varnish, by which he was enabled to take copies from engravings and of paintings, and also to take etchings from the images given by the camera obscura. Specimens of the etchings were shewn.

Dr Fyfe afterwards exhibited specimens of Photographic impressions, taken on pannell board by the phosphate, and also on copper, by the latter of which he stated, that the labours of engravers would in future be much diminished. He also explained an important practical application of the Photographic process to lithography, by which lithographers were not put to the trouble of tracing the object to be taken on the transfer paper. Lithographic specimens made in this way were exhibited.

Thanks voted to Dr Fyfe, who was requested to give in writing the results of his experiments.

Dr Douglas Maclagan also exhibited specimens taken by him; and another gentleman stated, that he had taken impressions on cloth, and afterwards washed out the remaining solution of silver.

3. Notice of a new method of re-shipping a Rudder at Sea. By Martyn J. Roberts, Esq. of Bryn-y-Caerau, Wales, Cor. Memb. R.G.S. of Cornwall, M.L.G.S., &c. India Street, Edinburgh. A model was exhibited. (618.) Referred to a Committee.

4. Suggestions of Improvement in Cloth Binding, from practical experience. By Mr William Bulloch, North Bank Street, Edinburgh.

A Model of the Sliding Frame and Moveable Stamp were exhibited. (629.) Referred to a Committee.

5. Donation.—Transactions of the (London) Society for the Encouragement of Arts, Manufactures, and Commerce, during Session 1837-8. Vol. LII. Part. I. From the Society. (634.) Thanks voted.

6. Read and approved the Experimental Committee's Report upon Dr Davidson's Papers:

1. On Adulteration of Fixed Oils. (554.)
2. On Decolorization of Palm Oil. (555.)
3. On removing the Fetid Odour of Fish Oils. (556.)
4. On removing the Bitter Taste and Lichenous Odour of Iceland Moss, &c. (557.)

The following Candidates were admitted as Ordinary Members, viz.:—

1. Henry Edward Dibdin, Esq., 72 Northumberland Street; 2. John Spence, Esq., 50 Great King Street; 3. Mr William Casey, Castle Mills, Fountainbridge; 4. Mr Thomas Ellis, Charles Street.

17*th April*.—David Maclagan, M.D., F.R.S.E., C.S.A. in the Chair. The following communications were laid before the Society:—

1. Notice of recent improvements effected by him in Photographic Drawing, &c. whereby the lights and shadows are not reversed. By An-

drew Fyfe, M.D., F.R.S.E., Vice-Pres. Soc. Arts. (641.) Specimens were exhibited.

Before proceeding to describe his method of taking impressions without having a reverse, Dr Fyfe stated that he had received communications from several gentlemen, mentioning that they had repeated his experiments with the phosphate of silver paper, and also with the ammonia, as a preservative. With regard to the latter, it was found by them all to prevent the farther action of the light on the specimens, provided it was properly applied. As a test of its proper application, he stated, that the best method is to put the paper into a diluted solution of the ammonia, and to leave it for a short time, till all the yellow parts of the impression become white, shewing that the whole of the yellow phosphate is washed out. An impression on paper, taken by Mr Wm. Forrester, lithographer, was shewn. In this the lights and shadows were preserved as in the original, by covering the light parts of the drawing from which it was taken with a dark ground, and leaving the darker parts lighter and lighter, so as to allow the greater transmission of light through what, in the original, was the darkest. A lithographic stone was likewise shewn, on which an impression was taken by Mr Nichol, lithographer, by covering the stone with phosphate of silver, and then, after putting an engraving on it, exposing it to light in the usual way. Dr Fyfe then proceeded to describe the process by which he had succeeded in getting impressions, in which the lights and shadows are *not reversed.* For this purpose the phosphate paper is first darkened by the action of light; it is then immersed in a solution of the iodid of potassium, and, while still moist, exposed to light, with the object, the impression of which is to be taken, placed on it, and left till the whole of the paper exposed becomes yellow, and when removed it exhibits a distinct representation of the object. In this process there is a tendency of the iodid to convert the dark phosphate to yellow iodid of silver, which it does instantly when the solution is strong, but very slowly when it is weak, unless it is exposed to light, and then the action goes on rapidly. It was observing this that induced Dr Fyfe to try the influence of light on phosphate paper besmeared with iodid of potass, by which he was led to the discovery. Of course, when an object, which allows the light to pass through it differently, is put on the paper, those parts on which the denser portions of the object are placed still retain their dark colour, the other parts are tinged, just according to the transmission of the light. When impressions thus prepared are kept, they gradually begin to fade, owing to the continued action of the iodid of potass, and hence the necessity of submitting them to a preservative process. After numerous trials, that which seemed to answer best, was merely immersing them in water for a few minutes, and in some cases even allowing a stream of water to flow gently on them, so as to wash out the whole of the iodid of potass not acted on—in this way the agent which tends to decolorize the blackened phosphate, seems to be removed. Several specimens of impressions from dried leaves were shewn, in which the different parts seem to be as nicely delineated, as in the other processes, where the reverse is given. Dr Fyfe, however, stated, that though they seemed to be preserved, yet he could not positively assert that they were so, as they had been prepared only a few days. He also mentioned, that, owing to the unfavourable state of the weather, he had tried the camera obscura only once, and though there was no sunshine, he had succeeded in getting a very faint delineation, from which he was in hopes that, in this way, a true representation may be obtained. He also stated, that impressions may be got by using darkened chlorid paper, in the same way. In this case the solution of iodid must be much weaker than that for the phosphate, the chlorid being more easily acted on. In both cases the solution is made of such strength, that when the paper is touched with it, it acts on the phosphate or chlorid feebly, and then, before being spread on, it must be diluted with a little water, so as just to have it of such strength that it does not act. A specimen of impression from chloride was also shewn.

The thanks of the Society were given to Dr Fyfe for this interesting communication.

2. Additional Notice regarding the late Mr William Blackie, optician, Upper Hermitage, Leith. Communicated by Mr David Dick, architect, Edinburgh. (640.) Thanks voted.

3. Part I. of an Essay on the present state of Art, as applied to British Manufactures, by Mr John Whyte, pattern-drawer, 39 Clerk Street, Edinburgh, was read. (601.)

4. Specimen of Printing in Relief for the Blind, with Architectural Embellishments. By John Alston, Esq. of Rosemount, Hon. M.S.A. (639.) Referred to a Committee.

5. Donation.—Description of Ramsden's Engine for dividing Straight Lines on Mathematical Instruments, 1779, with Plates. From Mr Edward Sang, F.R.S.E.—M.S.A. (635.) Thanks voted.

6. Donation.—Astronomical Observations made at the Royal Observatory, Edinburgh. Vol. II. for the year 1836. By Thomas Henderson, Esq. F.R.S.E. and R.A.S. From the Author. (638.) Thanks voted.

24th April.—Sir John Graham Dalyell, Kt. President, in the Chair. The following communications were laid before the Society:—

1. The President read a Letter received from Cav. Gio. Batta. Amici, Astronomer to the Grand Duke of Tuscany, Florence, Hon. Memb. Soc. Arts, prefixing a Circular inviting the Members to attend a general Scientific Meeting, to be held at Pisa in October next.

2. Part II. of an Essay on the present state of Art as applied to British Manufactures. By Mr John Whyte, Pattern-Drawer, 39 Clerk Street, Edinburgh, was read. (601.)

3. Description of a New Method of Propelling Steam-Vessels. By Mr D. T. Hope, 34 Buccleuch Place, Edinburgh.—A Model was exhibited. (642.) Referred to a Committee.

4. Description and Drawings,—1st, Of an Improvement on the Common Smith's Vice:—and, 2dly, Of an adjusting Drill Chuck, suitable for Turning Lathes, or Vertical Boring Machines. By Mr James Marshall, Engineer, 4 Coupar Street, North Leith. Models were exhibited. (637.) Referred to a Committee.

15*th May.*—Andrew Fyfe, M. D., F.R.S.E., Vice-President, in the Chair. The following communications were laid before the Society:—

1. Through the kindness of Mr Cooper, a Model of Mr Russell's Parallel Motion for the Steam-Engine was exhibited. Thanks voted.

2. Part III. of an Essay on the present state of Art, as applied to British Manufactures. By Mr John Whyte, Pattern Drawer, 39 Clerk Street, Edinburgh. (601.) Thanks voted.

3. Description and Model of Mr Hutchison's mode of heating Fermenting Liquids in very cold Weather. Communicated by Dr D. B. Reid, F.R.S.E., M.S.A. (644.) Thanks voted for the Models.

4. Notice of the Chemical Abacus, used for facilitating the study of Theoretical and Practical Chemistry. By Dr D. B. Reid. (645.) Thanks voted.

5. Notice of Long's American Frame Bridge. By David Stevenson, Civil Engineer, M.S.A. (648.)

> Working drawings, and a model constructed by Messrs Maxton of Leith, of a bridge on this principle, 150 feet span, to be erected in India, were exhibited. It was remarked by an eminent architect who was present, that the principle on which the framing of this bridge is constructed, might be applied with great advantage to many other purposes, where great lightness, combined with strength and rigidity, are requisite.
>
> Thanks voted to Mr Stevenson for his communication, and also to Messrs Maxton for their construction of the model. To be printed in the Transactions, with a Plate, if Mr Stevenson agrees to it.

6. Donations.—Mr Alston of Rosemount, Hon. M.S.A., attended, and presented his latest Specimens of Printing for the Blind, being a copy of the Book of Genesis, intended as a commencement to the printing of the whole Bible for the use of the Blind. (650.) Mr Alston also presented two other books for the blind. (652) and (653.)

> Thanks voted to Mr Alston for his donations, and for his continued zeal and gratuitous exertions in the cause of the Blind.

7. Donation.—On the Colour of Steam under certain circumstances; and on the Colours of the Atmosphere. By Professor Forbes, F.R.SS. L. & E.

From the Trans. of the Royal Soc. of Edin. From the Author. (649.) Thanks voted.

8. Donation.—Specimen of Lithographed Photography: which was lithographed, transferred, and printed, within two hours after the Photographic impressions were exhibited to the Society, at the meeting on 17th April 1839. By Mr William Forrester, Lithographer, 14 George Street, Edinburgh. (643.) Thanks voted.

9. The following Reports of Committees were read and approved of, viz.:—

(1.) On Mr Roberts' Method of Re-shipping a Rudder at Sea. Mr R. Hunter, Convener. (618.)
(2.) On Mr Bulloch's Suggestions of Improvement in Cloth Binding, and Sliding-frame. Mr T. Greig, Convener. (629.)
(3.) On Mr Gilchrist's Substitute for Door Springs. Mr Slight, Convener. (631.)
(4.) On Mr D. T. Hope's Paddle-wheel. Mr Maxton, Convener. (642.)
(5.) On Mr Alston's Printing in Relief, with Architectural Embellishments for the Blind. Mr Fraser, Convener. (639.)

Mr Thomas Menzies, Ship-builder, Leith, was admitted an Ordinary Member.

29th May.—Sir John Graham Dalyell, Kt., President, in the Chair. The following communications were laid before the Society:—

1. Notice of a cheap and simple method of preparing Paper for Photographic Drawing, in which the use of any salt of *silver* is dispensed with. By Mungo Ponton, Esq., F. R. S. E., For. Sec. Soc. Arts. (654.) Specimens exhibited. The method described by Mr Ponton is to steep the paper in a saturated solution of bichromate of potass, and then to dry the paper quickly at a brisk fire, excluding daylight. This salt is very much cheaper than the salts of silver, and is sensitive enough for lithographic and other practical purposes, giving a fine yellow figure of the object on a dark orange or tawny ground. To fix it, all that is necessary is to immerse the paper in water, which acts on the part of the paper which had not been exposed to the light, while it has no effect on the dark ground which had been exposed to the sun's rays. The effect is to deprive the yellow of a great part of its colour, and to turn it to stone or cream colour. Thanks voted, and to be printed in the Transactions.

Dr Fyfe was also requested to give a written notice of his Photographic Experiments, to be printed in the Transactions.

2. Sir John Robison described verbally the appearance of M. Daguerre's Photographic Pictures, which he had recently an opportunity of examining with much minuteness when in Paris. (656.) Thanks voted, and Sir John was requested to give a written notice on this subject, to be printed in the Transactions.

3. Sir John Robison also shewed a specimen of a new mode of Casting

in Plaster of Paris, which has recently been invented in Paris. The specimen (the hand of a child) was much admired. (657.) Thanks voted.

4. Description, with a Drawing, of a Method of filling the Boilers, either of Land or Marine Engines, which may be done either when the engine is, or is not, in motion. By Mr John T. Rose, Cromwell Street, North Leith. (575.) Referred to a Committee.

5. Description and Drawing of an improved mode of filling the Boilers of Steam Engines with Water. By Ditto. (579.) Referred to a Committee.

6. Letter from Mr James Whitelaw, formerly in London, now in Glasgow, about his Plan for feeding Boilers (553), with four Numbers of the Mechanics' Magazine, in which he refers particularly to a modification suggested in the P. S. to his description in No. 769 of that Magazine. (576.) Referred to a Committee.

7. Descriptions and Drawings of a method of increasing and diminishing the Distance of the Floats from the centre of the common Paddle-wheel; and of three modes of filling Steam Boilers with Water. By Mr D. T. Hope, 34 Buccleuch Place, Edinburgh. Referred to a Committee.

8. Descriptions and Drawings, 1st, Of a method of Working the Valves of a Steam Engine, which combines the simplicity of the excentric motion with the advantages of the hand-gear engine (619); 2d, Of an Universal Joint of a new construction (620); and, 3d, Of an improvement on Friction Cones. (621.) By Mr James Whitelaw, Engineer, 4 Wellington Street, Glasgow. Referred to a Committee.

9. Letter regarding his mode of Lighting Shop Windows, and his Inside Sashes for such windows (182), formerly submitted to the Society. By Mr Thomas Johnston, 137 George Street, Glasgow. (630.) Referred to a Committee.

10. A new method of Saving Life and Property at Duddingston Loch and Water of Leith. By Mr John Baillie, 28 Cumberland Street. (633.) Thanks.

11. Description of a Self-acting Water-Closet, with Ball-Cock. By Messrs John Bryden and Sons, Bell-hangers, 80 Rose Street, Edinburgh. A working model was exhibited. (646.) Referred to a Committee.

12. Description of a Bank-Safe Lock. By ditto. The Lock was exhibited. (647.) Referred to a Committee.

13. Donation.—Transactions of the Institution of Civil-Engineers, London, Vol. III. Part I. From the Institution.—Thanks voted.

14. The following Report of Committees were read, viz:—

(1.) On Captain Hay's method of Stopping Leaks in Steam Boilers. Mr Maxton Convener. (602.)

(2.) On Captain Hay's Lute for Steam Pipes. Mr Maxton, Convener. (603.)

(3.) On Mr Sang's]Waving Bar for Ruling Machines. Sir John Robinson, K.H., Convener. (615.)

(4.) On Mr Macpherson's Stereotype Mould-lifting Machine. Mr Russell, Convener. (610.)

(5.) On Mr Baillie's Frame for cleaning Windows. Mr Wright, Convener. (589.)

3d July.—Sir John Graham Dalyell, Kt., President in the Chair. The following communications were laid before the Society:—

1. Additional Verbal Remarks on Photography. By Andrew Fyfe, M. D.,F.R.S.E., V.P.S.A.—Thanks voted. (658.)

2. Farther Verbal Notice and Model of a perfect Parallel-motion applied to the Marine Steam-Engine. By John Scott Russell, M.A., F.R.S.E., V.P.S.A.—Thanks voted. (655.)

3. Notice of Palmer's improved Table Lamp, which is said to give a better and steadier light with common Whale Oil than is given by the common Lamp with Sperm Oil. By James Tod, Esq. Secretary. (661.) A Drawing was exhibited.—Thanks voted. Dr Ure has stated that his lamp burns well for fourteen hours, and gives light equal to eleven wax candles, three to the lb.

4. Model and Description of his Patent process of generating Steam by means of a heated medium. By George Goodlet, Esq. Leith.—Thanks voted. (659.)

5. Verbal Notice of a substitute for the Fly-Wheel in Steam Engines. By William Buckle, Esq. Soho. Communicated by John Scott Russell, M.A., F.R.S.E., V.P.S.A.—Thanks voted. (591.)

6. Model and Description of a Tool for setting Saw Teeth. By Mr James Ormiston, Wright (with Mr Beresford, Macbiehill,) Peeblesshire, Communicated by Robert Maxton, Esq. M.S.A.—Referred to a Committee. (660.)

7. The following Reports of Committees were read and approved of, viz.—

(1.) On Mr Hope's Expansion Railway. Mr D. Stevenson, Convener. (584.)

(2.) On Mr Roberts' Assaying by Galvanism. Mr Ponton, Convener. (611.)

(3.) On Mr Meiklejohn's Railway-crossing. Mr R. Hunter, Convener. (612.)

(4.) On Mr Hope's Method of increasing and diminishing the distance of the Floats from the centre of the Common Paddle-Wheel. Mr Scott Russell, Convener. (583.)

(5.) On Mr Whitelaw's mode of working the Valves of the Steam-Engine. Mr Sang, Convener. (619.)

(6.) On Mr Whitelaw's new Universal Joint. Mr Sang, Convener. (620.)

(7.) On Mr Whitelaw's Improved Friction Cones. Mr Sang, Convener. (621).

(8.) On Mr Johnston's mode of Lighting Shop Windows. Dr Fyfe, Convener. (630.)

(9.) On Messrs Bryden's Self-acting Water-Closet, with Ball-Cocks Mr Crawfurd, Convener. (646.)

(10.) On Messrs Bryden's Bank-Safe Lock. Mr Crawfurd, Convener. (647.)

8. The following Committees were requested to report on or before 1st August next. Their reports to be sent to Prize Committee, viz.—

(1.) On Mr St Clair's method of augmenting the Tone of the Violin. Sir J. G. Dalyell, Knt. Convener. (588.)

(2.) On Mr Scott Russell's Parallel Motion for the Steam Engine. Mr Slight, Convener. (626.)

(3.) On Mr Rose's Method of Feeding Steam Boilers. Mr W. Steele, Convener. (575.)

(4.) On Mr Rose's Improved Ditto. Mr W. Steele, Convener. (579.)

(5.) On Mr Whitelaw's Method of Feeding Boilers. Mr W. Steele Convener. (576.)

9. The following Reports to be lodged next session, after proper experiments, shall have been made, viz.,

(1.) On Mr Kirkwood's Stove. Mr Ponton, Convener. (623.)

(2.) Mr Brown's Paper on the Constitution of Steel. Experimental Committee, Mr Ponton, Secretary. (628.)

1. The following Candidate was admitted as an Ordinary Member, viz.:—Alexander Boyd, W. S., 28 St Andrew Square.

2. M. Daguerre (of Paris) officier de la Legion d'Honneur, the ingenious inventor of a peculiar method of producing accurate Photogenic Pictures, was proposed by the Council for election as an Honorary Member, and was admitted unanimously.

3. Copies of the List of Prizes adopted by the Society, for Communications to be read during the Session 1839–40, were distributed and ordered to be advertised as usual.

4. The Society elected the following Committee of twelve Ordinary Members to award the Prizes for the current Session 1838–39, viz.—Sir John Graham Dalyell, kt., President; Lieut.-Col. Blanshard, R.E., Dr Maclagan, Professor Forbes, Mr Slight, Mr Maxton, Mr Whytock, Mr Wighton, Mr C. H. Wilson, Mr Macgibbon, Mr Wright, Mr Brown. The Secretary, *ex officio, Convener.*

5. The Society appointed a Committee to purge the List of Member in Arrear.

After an appropriate address by the President, in which he took notice of the flourishing condition of the Society, both as regards the excellence of the communications, and the increasing number of its members, the Society adjourned till next Session.

List of Prizes for Session 1839–40.

The Society for the Encouragement of the Useful Arts in Scotland, announce the following Prizes for Session 1839–40.

I. For the most important Invention, Discovery, or Improvement in the Useful Arts;—*The* KEITH MEDAL, *value Twenty Sovereigns.*

II. For the best Essay, to be lodged betwixt and 1st November next, on the most improved manner of laying Rails on Railways,—the most improved form of Rails,—and the best width of Gauge; —*The* SOCIETY's GOLD MEDAL, *value Twenty Sovereigns,*

III. The Society proposes also to expend the sum of EIGHTY SOVEREIGNS, in various Pecuniary Prizes, and Honorary Medals, in rewarding approved Communications on the subjects hereafter mentioned,—and likewise on any other Inventions or Improvements which may be submitted to them,—and for Essays or detailed Accounts of Public or other Undertakings of great National importance, not previously published.

1. For the best series of Experiments applicable to the Useful Arts.
2. For the most important Communication of any useful Invention, Process, or Practice from foreign countries, not yet known or adopted in Great Britain.
3. For the best set of Experiments on the Consumpt of Fuel by Locomotive Steam-Engines.
4. For the best series of Experiments on the Comparative Economy, Comfort, and Safety as regards health, of Stoves for heating apartments.
5. For an account of a new and improved method of ascertaining Degrees of Temperature too high to be indicated by Common Thermometers; with a view to enable Founders, Bakers, and others who employ fire heat, to conduct their respective processes with greater certainty.
6. For the best series of Experiments on Photographic Drawing, particularly as to the Preserving and Multiplying of the impressions.

General Observations.—All communications shall be entitled to compete for the KEITH MEDAL which comply with the terms of the announcement of that Prize, although falling under any of the above specified subjects.

The descriptions of the various inventions, &c. to be *full* and *distinct*, and, when necessary, accompanied by *Specimens*, *Drawings*, or *Models*.

The Society shall be at liberty to publish in their Transactions, copies or abstracts of all Papers submitted to them. All Models, Drawings, &c. for which Prizes shall be given, shall be held to be the property of the Society; and these and all others which shal be approved of, shall be entitled to a place in the Museum.

All communications must be written on *Foolscap* paper, leaving margins at least one inch broad on both the *outer* and *inner* sides of the page, so as to allow of their being afterwards bound up with others; and all Drawings must be on *Imperial* Drawing Paper, unless a larger sheet be requisite.

The Society reserve to themselves the power of determining whether any Communication be of sufficient merit to entitle it to the Prize for which it competes, and of modifying the amount of the Prize.

All communications (except those competing for Prize II.) to be lodged *as soon after* 1*st November* 1839 *as possible*, in order to insure their being read during the Session; *but those which cannot be lodged so early, will be received till* 1*st March* 1840.

Communications, Models, &c. to be addressed to James Tod, Esq. the Secretary, at the Museum of the Society of Arts, 63 Hanover Street, Edinburgh.

Royal Institution, Edinburgh, 3*d July* 1839.

NEW PUBLICATIONS.

1. *Memoirs of the Wernerian Natural History Society, for the years* 1837–8. Part I. Vol. viii. With Five Engravings. 8vo, pp. 163. Edinburgh, Adam and Charles Black: Longman, Orme, Brown, Green, and Longman, London: and H. Beilby, Birmingham.

This first part of the eighth volume of the Wernerian Memoirs contains the following communications:—(1.) *Observations on the Distinctions, History, and Hunting of Seals in the Shetland Islands.* By Laurence Edmonstone, M. D.—This is the most interesting and amusing account of the British Seals hitherto published: it will be perused with equal pleasure by the general reader and the man of science. Dr L. Edmonstone, who is a native of Shetland, and resident in that secluded but very interesting country, we trust, will next take up the osteology and internal structure of the Phocidæ.

(2.) *On the last Changes in the relative Levels of the Land*

and Sea in the British Islands. By James Smith, Esq. of Jordanhill, F.R.S. L. & E. To which is appended, a Catalogue of recent Shells in the Basin of the Clyde and North Coast of Ireland;—Catalogue of Shells from the newer Pliocene Deposits in the British Islands, and of recent Shells of new species from the Firth of Clyde. With two Plates.—This valuable and important memoir is, by the Council of the Wernerian Society, recommended to the notice of those who may compete for the honorary prize offered by the Society for an approved Essay on the *so-called* Raised Sea-Beaches. The intelligent author of the memoir contends, that the beds of fossil shells described by him go to prove that the land has risen, an opinion, the plausibility of which we have often called in question, believing that the more ancient opinion—the *Lake hypothesis*—is competent to explain much more than some geologists give it credit for.

(3.) *On the Asteriadæ of the Irish Sea.* With two plates. By Edward Forbes, Esq., M.W.S.—This we consider to be the best monograph on our asteriadæ hitherto published.

(4.) *Meteorological Journal for the year* 1838. *Kept at the Manse of the Parish of Abbey St Bathans, Berwickshire. Lat.* 55° 52′ *N., Long.* 2° 23′ *W., at the height of about* 450 *above the Sea.* By the Rev. John Wallace.—The author of this Journal is well known as a learned mathematician and an accurate observer; and, as his instruments are good, the details are worthy of implicit credit.

(5.) *On the Geognosy of the Isle of Eigg.* With a coloured plate. By R. J. Hay Cunningham, Esq., M.W.S.—This small but curious island exhibits in the rocky eminence named the *Scuir*, one of the most striking (if not the most striking) displays of columnar rock hitherto met with in Europe. The geognostical position of the porphyry of the Scuir, left in doubt by Oeynhausen and Dechen, the two celebrated Prussian geologists, has been fully made out by Mr Cunningham, who ascertained that it forms a great vein. This isle is also remarkable for its pitchstones—its sandstone, disposed like trap in globular concretions,—and its fossil organic remains.

Appended to this part, is the List of Subjects proposed for Honorary Premiums, by the Wernerian Nat. Hist. Soceity.

2. ***Journal of the Asiatic Society of Bengal.*** **Edited by James Princep, Esq. F.R.S., Secretary of the Asiatic Society of Bengal, &c. &c.**

The Number for December 1838, published on the 2d March 1839, contains the following articles connected with physical science:—On the Spontaneous Heating of Brine: By G. A. Princep, Esq. Short notice of the coast-line, rivers, and islands adjacent, forming a portion of the Mergui Province, from a late Survey by Captain R. Lloyd, Indian Navy. On the genus Hexaprotodon of Dr Falconer and Captain Cautley: By J. M'Clelland, Assist.-Surgeon, Bengal Service. Meteorological Register, kept at the Assay Office, Calcutta, for the month of December 1838.—This number, which is the 84th, concludes vol. vii. of this valuable periodical. The Editor, we regret to learn, is forced to leave India owing to indisposition. We trust, however, that one to whom the science of India is so deeply indebted, will ere long be restored to his wonted health; for whether in Europe or in India, we believe he will continue to labour for the furtherance of every thing connected with the science and arts of our Great Eastern Empire.

3. ***The Quarterly Journal of Agriculture, and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland.*** **8vo, in Quarterly numbers. Edinburgh, William Blackwood and Sons; London, Thomas Cadell; Dublin, William Curry jun. and Co.**

An important department of this excellent and ably conducted journal, has hitherto been but little noticed; we allude to that portion of it devoted to the geognostical and geological Prize Essays of the Highland and Agricultural Society of Scotland. These essays, we are confident, when more generally known, will be found worthy the particular regard not only of geologists, but also of those who study agriculture with liberal and enlightened views. Already the following essays, with accompanying coloured geognostical maps and sections, have been published.

(1.) *Geological Survey of Berwickshire: with a coloured geological map.* By David Milne, Esq. F.R.S.E., F.G.S., Advocate.

(2.) *Report on the Geology of the East of Fife Coal-Field:*

with a coloured geological map and numerous sections. By Mr David Landale, Coal-Engineer, Wemyss, Fifeshire.

(3.) *On the Geology of Morayshire: with a coloured geological map.* By Mr John Martin, Elgin.

(4.) *Outlines of the Geology of Renfrewshire and the North of Ayrshire: with a coloured geological map.* By William Montgomery, Esq. younger of Cloak.

(5.) *Outline of the Geology of the South-east District of Perthshire: with a coloured geological map, and coloured sections.* By George Buist, Esq. Cupar of Fife.

(6.) *Geognostical Account of the County of Sutherland: with a large coloured geognostical map and many sections.* By Robert James Hay Cunningham. Esq. Member of the Wernerian Society, &c.

4. ***Researches on the Development, Structure, and Diseases of the Teeth.*** **By Alexander Nasmyth, F.L.S., F.G.S, Member of the Royal College of Surgeons, London. 8vo, pp. 182, with seven highly finished plates. J. Churchill, Soho, London.**

Odontology, or the anatomical, physiological, and pathological history of teeth, has, till within a comparatively late period, been almost entirely in the hands of the mere dentist. The early and beautiful observations of Leeuwenhoek, published in the Philosophical Transactions for the year 1678, which may be considered as having paved the way for the interesting researches of modern investigators, were forgotten or almost entirely neglected, until the subject was taken up by two able naturalists Retzius, a Swede, and Purkinje, a German, who, unknown to each other, resumed the course of investigation began by Leeuwenhoek, by a series of observations upon the structure of the teeth, with the assistance of the microscope. Müller of Berlin, our celebrated comparative anatomist Owen, and an acute and accurate observer, Mr John Goodsir jun. of Anstruther, by pursuing the same path, have still farther extended our acquaintance with the structure and physiology of the teeth.

Mr Alexander Nasmyth (brother of the well known and accomplished Edinburgh odontologist), the author of the very valuable volume now before us, has also contributed many new and important original observations. The "*Researches*," which ought to be in the hands of every naturalist and medical practitioner, may be regarded as an introduction to the more extensive work on odontology promised by our author.

5. *The Collected Works of Sir Humphry Davy, Bart.* Edited by his brother JOHN DAVY, M.D., F.R.S. 8vo. Smith, Elder and Co., Cornhill, London. 1839.

Two volumes of this valuable work are now before the public. The first volume contains "Memoirs of the Life of Sir H. Davy," by far the best biography of Davy hitherto published, and worthy the high name of its accomplished author Dr John Davy. The second volume contains early miscellaneous papers from 1799 to 1805; with an introductory lecture, and outlines of lectures on chemistry, delivered in 1802 and 1804, by Sir H. Davy. This volume is mainly interesting on two accounts: one, as illustrating the progress of chemical discovery, and more especially of voltaic electricity; the other, as displaying the progress of the author's own mind, and the formation of his literary character.

It is presumed that this new, uniform, and beautifully got up edition of the writings of Sir H. Davy, will not exceed ten volumes, embracing the whole of his works, which were published during the space of thirty years (1799 to 1829), a period memorable in the history of chemistry, and in no small part owing to his own discoveries.

6. *Memoir on the Mid-Lothian and East-Lothian Coal-Fields: with a Map and numerous Sections.* By DAVID MILNE, Esq. F.R.S.E., and F.G.S. 4to, pp. 152. Edinburgh, William Blackwood and Sons: T. Cadell, Strand, London. 1839.

Mr Cunningham's prize-essay on the Geology of the Lothians; the Geology of the Lothians and Fife, by Mr Maclaren; and this third valuable volume by Mr Milne, afford ample proof of the activity of our Edinburgh geologists, and also of the interesting nature of the Lothian portion of the Middle District of Scotland. This memoir is distinguished from those already mentioned, by the fulness of its details in regard to the geological and economical relations of the stratified rocks of the *coal-fields*. Our author has also been at great pains in collecting, from the best sources, numerous facts illustrative of the characters of our coal-mines, their mode of working, quantity of coal raised, and the moral and domestic condition of the collier population. To the miner and geologist, the numerous tables, drawn up with great care, will be received as useful contributions. The map, although on rather too small a scale, is valuable. Throughout the memoir there is a good deal of ingenious discussion, intended, we believe, more for the speculative geologist than the miner. Mr Milne's estimate of Mr Williams, the author of the "*Mineral Kingdom*," and of some other labourers in this geological field, we hold to be incorrect.

7. *Zoology of the Voyage of H. M. S. Beagle, under the command of Captain Fitzroy, during the years* 1832 *to* 1836. 4to. Smith, Elder, and Company, London.

The number of this work now before us, the last published, is No. IV. of Part II. of the Mammalia, which completes the natural history of the mammiferous animals met with during the voyage. The natural history of the species of the genus Mus, part of which was given in a former part, is here finished, twenty-eight species being described, and beautifully figured. To these follow the natural history of three species of the new murine genus, named Reithrodon. Species of several genera of the sections Hystricina and Leporina are described, and with these this first and elegant volume of the zoology of the Beagle is finished.

8. *An Etymological and Explanatory Dictionary of the Terms and Language of Geology.* By GEORGE ROBERTS, 12mo, pp. 183. London, Longman, Orme, Brown, Green, and Longman, 1839.

This useful little volume we recommend to our young geological friends; even those advanced in the study may refer to it with advantage.

9. *Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology, and as a Guide to the philosophical pursuit of Natural History.* By W. B. CARPENTER,

M. D., Member of the College of Surgeons, London, &c. 8vo, pp. 478. Six Plates. John Churchill, Prince's Street, Soho, London.

This work is designed to afford to the student of any department of botany and zoology, or physiology, such a comprehensive view of the whole science of organized nature, and especially of its general principles, as may render the pursuit of any particular branch more interesting and profitable than if carried on without such preliminary knowledge. It differs from English elementary works on zoology and botany, in combining an outline of these divisions of the science (in which every leading natural group is treated on the same scale), with physiological details on the structure and life of the various forms of plants and animals; and it differs from most works on physiology, in deriving the facts of that science, on which generalizations are to be founded, from all classes of organised beings, vegetable as well as animal. It is properly remarked by the author, that just as the physical philosopher seeks to combine as many similar phenomena as he can discover, for the basis of his general principles, the physiologist should employ the different classes of living beings as so many groups of *instances*, by the analysis of which he may insulate the phenomena with more freedom from disturbing causes, than in pursuing his researches by experimenting on one kind of organization alone. The laws of life can only be searched for with a probability of success, by investigating their operations *wherever* presented to us; and no generalizations can be really valid, which are not applicable to *all* classes of living beings.

It is the author's object, then, in this volume, to give a connected summary of what is at present known on the subjects it embraces; including "whatever general principles may be regarded as firmly established, with as many facts as may serve to illustrate them, without distracting the attention by profuseness of detail." He has not as yet contributed much to the gigantic pile which the industry of observers has accumulated, and which is at present being so rapidly increased; but he has attempted, not without success, to reduce it to forms of greater beauty and harmony. "At every successive step in generalization," he remarks, "are we able to comprehend new relations between facts that previously seemed confined and insulated, new objects for what at first seemed destitute of utility." He has, moreover, frequently supplied links, where such were wanting, particularly in the department of vegetable physiology. The analogies between the vegetable and the animal kingdoms are pursued as far as they can be fairly traced at the moment; and it is shewn that the study of each is capable of throwing valuable light on difficult points in the phenomena of the other. The following is a general account of the contents of the volume. The introduction contains an account of organised structures in general, of the elementary tissues of plants and animals, and of the principal natural divisions of the two organised kingdoms. The first book is devoted to general physiology, and contains an exposition of the essential phenomena of life, and the laws by which they appear to be regulated. In the second book, under the head of special physiology, these phenomena are considered more in detail, and distributed according to their *functional* character. Each function, with the organs which perform it, is traced through its various manifestations in plants and animals, from the lowest to the highest of each scale; and its various forms are contrasted with the transitory conditions it passes through in the development of the highest of each kingdom. We may add, that the beautiful plates add much to the value of the work.

We understand that it is the author's intention to bring out before long a companion volume, more particularly devoted to Natural History.

List of Patents granted for Scotland from 25*th June to* 17*th September* 1839.

1. To Richard Beard of Egremont Place, New Road, in the county of Middlesex, gentleman, in consequence of a communication from a foreigner residing abroad, for an invention of "improvements in printing calicoes and other fabrics."—25th June 1839.

2. To John Small of Old Jewry, in the city of London, merchant, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in the manufacture of thread, or yarn and paper, by the application of certain fibrous materials not hitherto so employed."—26th June 1839.

3. To James Lees of Salem, near Oldham, in the county of Lancaster, cotton spinner, for an invention of "an improvement in the machinery for spinning, twisting, and doubling cotton, silk, wool, hemp, flax, and other fibrous materials."—2d July 1839.

4. To **John Arrowsmith** of Bilston, in the county of Stafford, civil-engineer, for an invention of "certain improvements in steam-engines."—3d July 1839.

5. To **Frank Hills** of Deptford, in the county of Kent, manufacturing chemist, for an invention of "certain improvements in the construction of steam-boilers and of locomotive-engines."—3d July 1839.

6. To **Thomas Clark** and **Charles Clark** of Wolverhampton, in the county of Stafford, iron-founders and copartners, for an invention "for glazing and enamelling cast-iron, holloware, and other metallic substances." —4th July 1839.

7. To **Alexander Gordon** of Fludyer Street, Westminster, in the county of Middlesex, engineer, for an invention of "a new machine or apparatus for employing or using steam or other elastic fluid as a motive power," the same having been communicated to him by a foreigner residing abroad.—5th July 1839.

8. To **James Kay**, formerly of Preston, in the county of Lancaster, cotton-spinner, but now of Pendleton, near Manchester, in the aforesaid county, flax-spinner, of an extension of three years, from 23d June 1839, of a patent granted to him for an invention of "a new and improved machinery for preparing and spinning flax, hemp, and other fibrous substances, by power." —11th July 1839.

9. To **Abraham Bury** of Manchester, in the county of Lancaster, gentleman, for an invention of "certain improvements in the mode of printing, colouring, or dyeing cotton or other fabrics."—12th July 1839.

10. To **Joseph Maudslay** and **Joshua Field**, of the firm of Maudslay, Sons, and Field, engineers, of Lambeth, in the county of Surrey, for an invention of "improvements in the construction of marine steam-engines, which are particularly applicable to steam-engines of the largest class."—15th July 1839.

11. To **Charles Sanderson** of Sheffield, in the county of York, steel manufacturer, for an invention of "a certain improvement in the art or process of smelting iron ores."—15th July 1839.

12. To **James Templeton**, manufacturer in Paisley, and also **William Quiglay**, weaver, in Paisley, for an invention of "machinery for a new and improved mode of manufacturing silk, cotton, woollen, and linen fabrics."—17th July 1839.

13. To **Pierre Auguste Ducote** of No. 70 St Martin's Lane, in the county of Middlesex, for an invention of "certain improvements in the art of printing on paper, calicoes, silks, and other fabrics."—17th July 1839.

14. To **John Thomas Betts** of Smithfield Bars, in the city of London, rectifier, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in the process of preparing spirituous liquors in the making of brandy."—19th July 1839.

15. To **David Johnston** of Glasgow, manufacturer, in consequence of a communication from a certain foreigner residing abroad, for an invention of "certain improvements in the manufacture of hinges."—19th July 1839.

16. To **Moses Poole** of the Patent Office, Lincoln's Inn, in the county of Middlesex, gentleman, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in printing calicoes and other fabrics."—19th July 1839.

17. To **John Fairrie** of Church Lane, Whitechapel, in the county of Middlesex, sugar refiner, for an invention of "improvements in making and refining sugar."—19th July 1839.

18. To **Henrick Zander** of North Street, Sloane Street, in the county of Middlesex, gentleman, for an invention of "improvements in steam-engines, steam-boilers, and condensers."—22d July 1839.

19. To **John Evans** of Birmingham, in the county of Warwick, paper manufacturer, for an invention of "improvements in the manufacture of paper."—23d July 1839.

20. To **John Alexander Elzear Degrand** of the Boulevard du Temple, in the city of Paris, but now of Covent-Garden, in the county of Middlesex, for an invention of "improvements in the production of motive power, and in machinery for applying the same to useful purposes."—23d July 1839.

21. To **Edward Francois Joseph Duclos**, late of Samson, in the kingdom of Belgium, but now of Leicester Place, Leicester Square, in the county of Middlesex, gentleman, for an invention of "improvements in the manufacture of zinc, copper, tin, and antimony."—23d July 1839.

22. To **Charles de Laveleye** of King's Head Court, Shoe Lane, in the city of London, engineer, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in the manufacture of bricks."—23d July 1839.

23. To **Theodore Cotelle** of the Haymarket, in the county of Middlesex, civil-engineer, for an invention of "improvements in extracting salt from sea or salt water, and rendering it pure and drinkable, and in purifying other waters."—23d July 1839.

24. To **William Newton** of the office for patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in engines to be worked by air or other gases."—31st July 1839.

25. To **Robert Griffiths** of Smethwick, in the county of Stafford, for an invention of "certain improvements in the construction of presses, which improvements are also applicable to the raising of weights."—1st August 1839.

26. To **Joseph Jennings** of Bisson Bridge, in the parish of Ker, in the county of Cornwall, assay master, for an invention of "a process for obtaining metal from pyrites or mundic."—1st August 1839.

27. To **Christopher Binks**, residing in Middlebie Street, Newington, near Edinburgh, for an invention of "certain improvements in obtaining or manufacturing chlorine, and certain compounds of chlorine, applicable in bleaching."—2d August 1839.

28. To **David Stead** of Great Winchester Street, in the city of London, merchant, partly a communication from a foreigner, and partly his own discovery, for an invention of "an improved mode or method of making or paving public streets and highways, and public and private roads, baths, courts, and bridges, with timber, or wooden blocks."—6th August 1839.

29. To **John George Shuttleworth** of the Mount, near Sheffield, in the county of York, soap-boiler, for an invention of "a new mode of obtaining a rotary motion from the rectilinear motion of the piston-rod of a steam, or other the like engine."—8th August 1839.

30. To **George Holworthy Palmer** of Surrey Square, Old Kent Road, in the county of Surrey, civil engineer, for an invention of "certain improvements in paddle wheels for propelling ships, boats, or other vessels, navigated by steam or other motive power."—16th August 1839.

31. To **John Rostron** of Edenfield, in the county of Lancaster, cotton

spinner, in consequence of a communication from a certain foreigner residing abroad, for an invention of "certain improvements in looms for weaving."—16th August 1839.

32. To WILLIAM HENRY HORNBY of Blackburn, in the county of Lancaster, cotton-spinner, and WILLIAM KENWORTHY of the same place, cotton-spinner, for an invention of "certain improvements in the machinery or apparatus for sizeing and otherwise preparing cotton, wool, flax, and other warps for weaving."—17th August 1839.

33. To JOHN MERCER of Oakenshaw, in the county of Lancaster, calico-printer and chemist, JOHN DYNELAY PRINCE the younger of Manchester, in the said county, calico printer, and WILLIAM BLYTHE of Church, in the said county, manufacturing chemist, for an invention of "certain improved processes to be used in the printing, dyeing, or colouring of cotton, woollen, silk, or other cloths and yarns."—17th August 1839.

34. To JOHN BUCHANAN of Glasgow, North Britain, coach-builder, and WILLIAM BRIDGES ADAMS of Porchester Terrace Bayswater, in the county of Middlesex, gentleman, for an invention of "certain improvements in the construction of wheel-carriages, parts of which improvements are also applicable to machinery for propelling, and also for the purpose of securing ships and other vessels, and for communicating motion between different portions of machinery."—22d August 1839.

35. To JAMES CAPPLE MILLER of Manchester, in the county of Lancaster, gentleman, for an invention of "certain improvements in printing calicoes, muslins, and other fabrics."—26th August 1839.

36. To BARCLAY FARQUHARSON WATSON of Lincoln's Inn Fields, in the county of Middlesex, solicitor, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in crushing or preparing New Zealand flax (Phormium Tenax.)"—4th September 1839.

37. To MATTHEW UZIELLI of King William Street, in the city of London, merchant, in consequence of a communication from a certain foreigner residing abroad, for an invention of "improvements in impregnating wood or timber with chemical materials."—4th September 1839.

38. To JOHN AUGUSTUS TULK of Seaton and Lowca iron-works, Cumberland, iron master, for an invention of "improvements in the manufacture of iron."—4th September 1839.

39. To FREDERICK PARKER of New Gravel Lane, Shadwell, in the county of Middlesex, animal charcoal manufacturer, for an invention of "improvements in revivifying or restoring animal charcoal."—6th September 1839.

40. To JOHN DICKSON of Brook Street, Holborn, in the city of London, engineer, for an invention of "improvements in rotatory steam-engines."—6th September 1839.

41. To WILLIAM HALE of Greenwich, in the county of Kent, engineer, for an invention of "improvements in steam-engines, and apparatus connected therewith, and in machinery for propelling vessels, part of which improvements are applicable to raising or forcing fluids."—7th September 1839.

42. To ROBERT CAREY of Breadgar, near Sittingbourne, in the county of Kent, gentleman, in consequence of a communication from a certain foreigner residing abroad, for an invention of "certain improvements in paving or covering streets, roads, and other ways."—17th September 1839.

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PLATE III. Edinr. New Phil. Jour. Vol. XXVII.

Sternoptix Celebes.

Br. Cr.	4
Br. R.	5
D.	1/10
A.	2/13
C.	18+18
P.	10
V.	1/7

Fig. 1.
(Nat. Size)

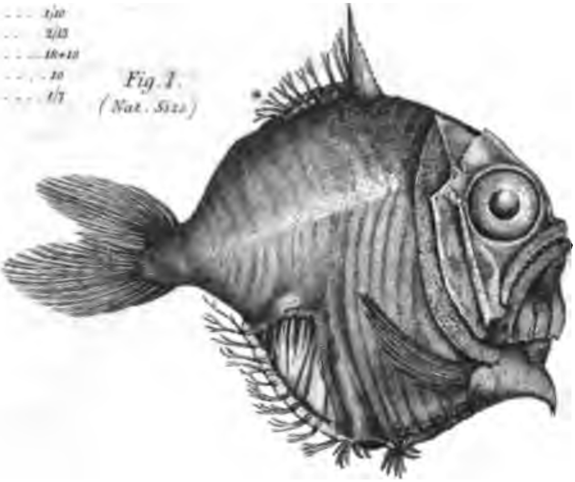


Fig. 2.
(Nat. Size)

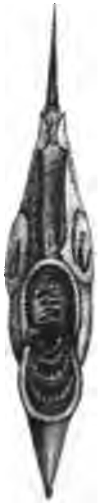


Fig. 3.
(Nat. Size)

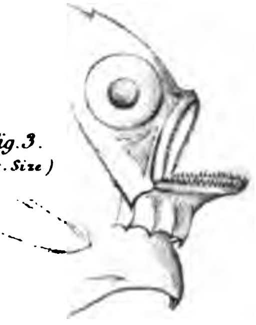


Fig. 4.

F. [illegible] Sc.

PLATE IV. Edinr. New Phil. Jour. Vol. XXVII.

Sternoptix Celebes, (one half larger than nature)

Fig. 1

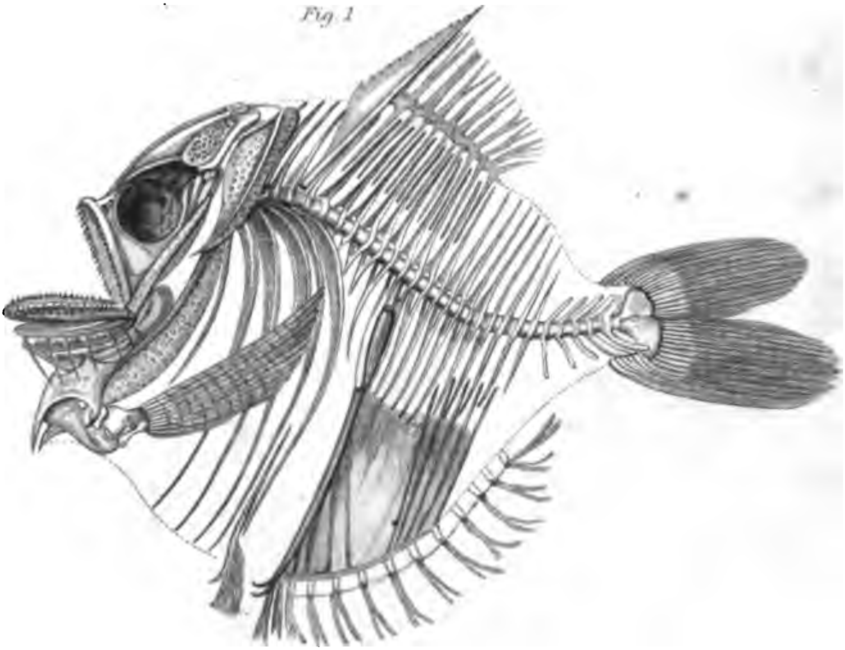
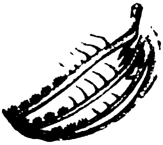
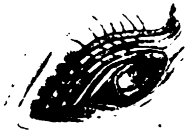


Fig. 2.



Lateral View

Fig. 3.



View of the Connexions Inferiorly & Superiorly

of the Left Branchial Arches & Palatine Teeth

(the Branchiæ being removed.)

E. Mitchell. Sc.